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PHILOSOPHICAL TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

LONDON.

VOL. LXXII. For the Year 1782.

PART I.

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THE Committee appointed by the *Royal Society* to direct the publication of the *Philoſophical Tranſactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations, which have been made in ſeveral former *Tranſactions*, that the printing of them was always, from time to time, the ſingle act of the reſpective Secretaries, till the Forty-ſeventh Volume: the Society, as a body, never intereſting themſelves any further in their publication, than by occaſionally recommending the revival of them to ſome of their Secretaries, when, from the particular circumſtances of their affairs, the *Tranſactions* had happened for any length of time to be intermitted. And this ſeems principally to have been done with a view to ſatisfy the Public, that their uſual meetings were then continued for the improvement of knowledge, and benefit of mankind, the great ends of their firſt inſtitution by the Royal Charters, and which they have ever ſince ſteadily purſued.

But the Society being of late years greatly inlarged, and their communications more numerous, it was thought adviſeable, that a Committee of their members ſhould be appointed to reconſider the papers read before them, and ſelect out of them ſuch, as they ſhould judge moſt proper for publication in the future *Tranſactions*; which was accordingly done upon the 26th of March 1752. And the grounds of their choice are, and will continue to be, the importance and ſingularity of the ſubjects, or the advantageous manner of treating them; without pretending to anſwer for the certainty of the facts, or propriety of the reaſonings, contained in the ſeveral papers ſo publiſhed, which muſt ſtill reſt on the credit or judgment of their reſpective authors.

It is likewiſe neceſſary on this occaſion to remark, that it is an eſtabliſhed rule of the Society, to which they will always adhere, never to give their opinion, as a body, upon any ſubject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently propoſed from the chair, to be given to the authors of ſuch papers, as are read at their accuſtomed meetings, or to the perſons through whoſe hands they receive them, are to be conſidered in no other light than as a matter of civility, in return for the reſpect ſhewn to the Society by thoſe communications. The like alſo is to be ſaid with regard to the ſeveral projects, inventions, and curioſities of various kinds, which are often exhibited to the Society; the authors whereof, or thoſe who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the higheſt applauſe and approbation. And therefore it is hoped, that no regard will hereafter be paid to ſuch reports, and public notices; which in ſome inſtances have been too lightly credited, to the diſhonour of the Society.

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PHILOSOPHICAL TRANSACTIONS.

I. *Relazione di una nuova Pioggia, ſcritta dal Conte* de Gioeni *abitante della 3ª Reggione dell' Etna; communicated by Sir* William Hamilton, *K. B. F. R. S.*

Read November 8, 1781 *.

> *Volat per Mare magnum cinis decoctus, et terrenis nubibus excitatis, tranſmarinas quoque provincias pulvereis guttis implevit.* CASSIOD. lib. IV. var. epiſt. 50.

LA mattina de 24 corrente ſi e qui preſentato uno de fenomeni piu' ſingolari; tutti li luoghi eſpoſti all' aria ſi trovarono bagnati da un' acqua colorita cretacea biggia, la quale evaporando indi, o infiltrandoſi nella terra, laſció per ogni dove la materia che contenea, all' altezza di due, o tre linee; tutti i ferri, che ne furono tocchi, divennero rugginoſi.

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* For a tranſlation of this paper ſee the Appendix.

Il publico portato al maraviglioſo, imaginó varie cagioni di tale pioggia, motivi di timore per gli veggetabili, e gli animali.

Le popolazioni, che ſi valgono delle acque piovane, ſi aſſennero di farne allora uſo. Vi fu' chi ſoſpetaſſe contenerſi in eſſa de' principi vitriolici: e delle perſone prediſſero un qualche male epidemico ſopraveniente.

Chi avea oſſervato l'eſploſioni, che l'Etna, da venti e piu' giorni, facea vedere dall' alto del ſuo cratere, inclinava a credere originato quel fenomeno da una di eſſe.

Si eſteſe la pioggia dal nord ¼ nord eſt al ſud ¼ ſud-oveſt ſopra le campagne di noto, fin dove contanſi in linea recta, ſettanta miglia, dal vertice dell' Etna.

Non e nuovo, che li volcani per la forza eſpanſiva, che violenta in eſſi ſi genera, abbiano cacciato delle Sabbie * portate da venti di lontane reggione, e delle pietre †.

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* L'autorita di Caſſiodoro. premeſſā a queſta relàzione e avvalorata da Seneca nel ſuo 2° lib. delle Queſt. Nat.

Ætna aliquando multo igne abundavit, ingentem vim arenæ urentis effudit, involutus eſt dies pulvere, populoſque ſubita nox terruit.

Ma ſenza riandare alle moltiſſime memorie di queſto volcano, e del Veſuvio, come a noi piu' vicini, abbiamo da 20 anni in qua veduto molte delle pioggie in Sicilia, originate dell' Etna, e l'ultima precedette la irruzione dell' anno ſcorſo, era quella compoſta di piccioli frammenti di pomici bituminoſe, o ſiumie.

† La pietra, deſcritta da PLINIO caduta nella Tracia, la pioggia di pietra, che avvenne ſul monte Albano, dopo la rovina di Alba, della quale ci fa menzione TITO LIVIO, e molte altre ſimili, rimarcate dalli antichi, come pioggie prodigioſe, ſono ſtate riconoſciute per volcaniche; in quanto all' Etna abbiamo a giorni noſtri veduto formare de' monti nuovi, per il cumulo delle pietre, o per meglio dire delle lave; e degli antichi, oltre STRABONE, ed altri molti ſcrittori, il lirico PINDARO ci traſmette, che, *aliquando non tantum rivos igneos ejecit, ſed ſaxa ignita.* PIND. ap Brit. lib. V. c. 14. p. 2.

Ma il colore della materia, e la ſua ſottigliezza, diedero motivi di dubitare, onde ne foſſe originata, accreſcendoſene l'incertezza dalla rimarchevole circoſtanza dell' acqua, che portolla incorporata * e peró ſi ſoſpettava di altro principio.

Era dunque vopo per ogni ragione di aſſicurarſi della natura di tale materia, onde reſtar perſuaſi della ſua origine, e degli effetti, che avrebbe potuto cagionare: non potea queſto farſi ſenza il ſoccorſo dell' analiſi chimica: per far dunque ció con ſiccurezza, procurai raccogliere quella pioggia in luogo, ove poteſſi credere, che non eſiſteſſero altre ſoſtanze eterogenee: Scelſi percio' la pianta chiamata *Braſſica Capitata*, la quale avendo le foglie larghe, e ravvolte, trovai, che contenea a ſufficienza dell' acqua colorita; e riverſate molte di eſſe in un vaſo, laſciai indi, che ſpontaneamente ne riſedeſſe la terra al fondo, in cui dopo qualche tempo depoſe la parte limoſa, reſtando l'acqua traſparente.

Separata queſta in altro vaſo, la tentai con de' liquori alcalini vegetabili, ed acidi minerali: ma non oſſervai decompoſizione con alcuno de' due meſtrui; paſſai ad evaporarle per riunire quelle materie, che potevano forſe eſſere in ſoluzione, e toccatala di belnuovo con gli anzidetti liquori, fece vederſi leggiera efferveſcenza con gli acidi; provata con lo ſiroppo di viole, divenne queſto verde ſmorto, coſi che mi perſuaſi, che conteneſſe un ſale calcareo +. Con la decozione di galla non produſſe precipitazione.

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* Ne' molti ſcrittori del noſtro Etna, non trovaſi Pioggia di Sabbia, o di altra produzione meſcolata con acqua.

+ Provata ancora con diſſoluzione di piombo nell' acido vegetale, perdette il ſuo color naturale, e la ſua traſparenza, divinendo lattiginoſa. Io mi farei a credere, che ſii quello un'effetto delle particelle alcaline, e coſi ſpiegare la effloreſcenza, che moſſe ſopra li ferri eſpoſti all' aria.

Diſſeccata poſcia all' ombra la materia, ſi fè vedere una terra ſottiliſſima di color cretaceo, ma inerte per eſſere ſtata diluta della pioggia.

Penſai calcinarla ad un fuoco leggiero, e vi preſe il color di mattone; poſta indi in un crogiuolo, una porzione di queſta la paſſai a fuoco piu' violente, e perdette quaſi il colore acquiſtato; un altra parte di queſta provata a piu' gagliarda, e lunga calcinazione (onde poteſſi ſperarne la vitrificazione) reſto cio non pertanto, frolle, e diviſiſſima, ritornando al ſuo priſtino colore biggio.

Oſſervato con accuratezza il fumo delle trè calcinazioni, non diede alcuno colore, ne odore, per ſoſpettarſi de meſcolanza arſenicale o ſulfurea.

Avuta dunque quella materia in tre porzioni, calcinata a tre differenti gradi di fuoco, le preſentai una buona magnète, ma non aggi queſta nella prima, e nella ſeconda; nella terza pero' una leggiera attrazione, in aghi viſibili, reiterata piu' fiate, mi fe ſtabilire, che ſia in queſta terra un principio marziale nella forma metallica, e non in ſoſtanza vitriolica *.

Della natura dunque delle materie riconoſciute, ſi rilieva la loro origine volcanica, imperoche il ferro piu' che e' eſpoſto a violenta calcinazione, perdendo il principio flogiſtico, piu' ſi rende diviſo, e non puo queſto ſuccedere naturalmente, che nel gran focolare di un volcano. Il ſale calcareo eſſendo un ſal marino, combinato con ſoſtanze calcaree per via di fuoco violente † non puo' altrimenti eſſere compoſto, che nel volcano ‡.

Percio,

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* Imperochè non avrebbe altrimenti l'acqua prodotto effervеſcenza con gli acidi, ma l'avrebbe moſtrato con gli alkali, e nella triplicata calcinazione, ſi ſarà piu' toſto accreſciuto, che diminuito il colore roſſo.

† La combuſtione delle pietre da Calce puo' produrre, e' vero, la combinazione,

Percio, che appartiene alli temuti effetti, ſopra gli animali, e gli veggetabili, e noto a chiunque, l'uſo vantaggioſo che ritira la medicina dell' uno e l'altro, in quella ſteſſa forma che furono preparati nel gran laboratorio della natura.

Li veggetabili, che ſono nell' attuale horificazione, non moſtrano la menoma macerazione, come altre volte e avvenuto con le pioggie di Sabbia *.

Come poi quella produzione volcanica ſi ſia meſcolata all' acqua, puo cio concepirſi in varie guiſe.

L'Etna e ordinariamente attornato nella ſua media reggione di nuvole, le quali non ſempre oltrepaſſano la ſua ſommita, che ſi alza a 2900 paſſi † ſopra il livello del mare, cacciatane fuori quella materia, trovando ſottopoſte le nuvole, poté avvenire, che ſi foſſe meſcolata alle ſteſſe, e ſciolta poi in pioggia nella maniera ordinaria: puo altrimente conghietturarſi, che quel denſo fumo, che contenea la materia volcanica per la forza de'

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zione, onde riſulto il ſale calcareo, ma ſcorgeſi chiaramente, che non potea quella quantita altronde provenire che dal volcano.

‡ Molte, e replicate eſperienze ſopra li prodotti dell' Etna mi hanno perſuaſo, che il ſal marino ſii uno de principali e piu' abondanti meſtrui, che eccitano le efferveſcenze del noſtro volcano, o che ne ſii la baſe (come un' amico di molta cognizione mi ha fatto nuovamente riflettere) trovo del ſale calcareo nelle vecchie lave, del ſale comune, lo trovo ſublimato in ammoniaca nelle fenditure, e ne' ſpiragli delle nuove eruzioni; ma qui non e luogo á cio' che richiede un maggior volume, forſe appreſſo potro' meglio dirne in altra occaſione.

* Mi trovo aver replicatamente oſſervato, che le pioggie di Sabbia del noſtro monte per lo piu' compoſte di materie calcinate, e di piccioli criſtalli di Schorl, portano un cimento di particelle arſenicali, e ſulfuree, e qualche volta ſaline, che uniſce lo Schorl alle altre materie; coſiche ſe ne ingroſſano li granelli; qualche volta ancora ci e arrivata la pioggia calda a terra.

† La miſura, che ho tentato della perpendicolare del monte, mi e due volte riuſcita all' altezza deſcritta, non pero la do per certa, ſapendo che l'altimetria ha vopo di eſatti iſtromenti, e di reiterate oſſervazioni, che dovro comprovare ancora con il barometro a migglior commodo.

venti, foſſe traſportato nell' atmosfera, con la ſua rarefazione, ſopra quel tratto di paeſe *, e quindi raffreddandoſi ſi ſia condenzato tanto, che ſuperando il peſo dell' aria ſottopoſta, ſi abbia ſciolto nella pioggia colorita.

Io rimetto per altro a fiſici, a quali appartiene la cognizione degli aggenti della natura, lo eſame, e la ſpiega di tale fenomeno, limitandomi alle oſſervazioni, ed alle eſperienze di naturaliſta iniziato nella chimica, affine di concorrere con eſſe di qualunque merito ſiano, alla teoria de' volcani, e del globo †.

P. S. A 4 Maggio, Venerdi alle ore 21½ di Italia ſi e fatta ſentire una ſcoſſa di terra aſſai leggiera, nelle abitazioni che ſono attorno all' Etna, la quale piu' ſi reſe ſenſibile in qualche lontananza del monte, la ſua azione fu dal nord al ſud. Avea il volcano continuato le fiamme, e le eſploſioni, e la notte precedente, una colonna di fumo, compoſta di globi quaſi articolati, l'uno ſopra l'altro, ſi era alzata ſopra il cratere all' altezza dupplicata della montagna per quanto facea arbitrare la diſtanza di

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* Accio la addotta ipoteſi non ſembri eſagerata per la quantita di fumo, che deveſi ſupporre, io rapporto cio che fu oſſervato da CICERONE, cratere flamma erumpit, fumo mixta tam copioſo, ut dum Boreas ſpirat Melitam uſque per aerâ illum ſublimem propellat ad IX. millia paſſuum ſpatium. CIC. de Nat. Deor. lib. II.

† La Phyſique (dice il Sr. WALERIO nella ſua mineral. t. 2. Hidrol. 2. ſ.) eſt plus univerſelle dans ſes vûes, et plus philoſophique dans ſon examen, le phyſicien enviſage, raiſonne, explique, le naturaliſte regarde, ramaſſe, et range; celui-ci vous dira il exiſte tel corps dans la nature, il eſt fait, ſoit au dedans, ſoit au dehors de telle ou telle maniere, il eſt de tel ou tel regne, claſſe, ordre, eſpéce, variété; celui la pretendra vous expliquer les cauſes de ſon exiſtence, de ſes formes, et de ſes propriétés.

Appreſſo al Sr. WALERIO l'illuſtre Sr. LINNÈ nell' anal. tranſalp. anno 1740, ſſ. 2. coſi ſcrive; Phyſica eſt ſcientia de qualitatibus elementorum. Hiſtoria naturalis autem circa cognitionem corporum naturalium verſatur: il vero naturaliſta dev' eſſere iſtruito della ſiſica, e della chimica ancora, ma non conoſciamo ancor noi qui la diviſione delle due ſcienze.

22 miglia dal vertice per linea retta, in cui e queſta citta; duró quella tutto la notte perpendicolare, ſolche ſi avea ſtaccato uno de' globi, ed allungato all' oveſt della ſua cima; tratto tratto tutto l'interno della colonna, e della lingua prolungata di fumo venivano internamente illuminate da fuoco elettrico, che traſpariva roſſo cupo, eſtinguendoſi gradatamente dal baſſo all' alto, in due ſecondi.

Ha continuato il fuoco ſul cratere ſin' oggi 8 Maggio, rigettando delle moli infuocate, le quali, vagamente rotolando giu per il cono, hanno illuminato quella reggione, e ſi e verſata della lava dal cratere in qualche quantitá verſo l'oveſt nord-oveſt; ma non ha avuto queſta la forza di rompere li fianchi, o le pareti del volcano, a tal che ſiamo nel caſo di appropiarci quella memoria ſtorica. MARCO ÆMILIO C. AURELIO Coſſ. *Ætna mons terræmotu, ignes ſuper verticem late diffudit.* Jul. Obſequ. de Prodig. c. 89.

II. *Nova experimenta Chemica quæ ad penitiorem Acidi e Pinguedine eruti cognitionem valere videntur. Scribebat* D. Laurentius Crellius, Gulielmo Huntero, *M. D. S. R. S.*

Read November 15, 1781.

NON ſine maxima animi voluptate e Philoſophicarum Tranſactionum volumine noviſſimo percepi, meas litteras ad te miſſas, de experimentis chemicis referentes, non ſolum tibi haud diſplicuiſſe; ſed etiam illuſtriſſimæ Regiæ Scientiarum Societati abs te, ea qua es humanitate oblatas, honorificentiſſimis illius ſuffragiis eſſe ornatas. Quanti hunc inſignem in me collatum honorem faciam, quanta ſit mea in celeberrimos viros, ſumma benignitate de commentariolo meo qualicunque judicantes, reverentia, quantæ denique grates, quas tibi, vir celeberrime, debeam, non poſſum verbis ſatis demonſtrare. Perſuaſum itaque te habeas, oro rogoque, de mente mea tibi devinctiſſima; quam ut quoque, omni qua polleo, facultate teſter, ill. Reg. Soc. ſubjunxi huic epiſtolæ nova ea experimenta chemica, quæ ad penitiorem acidi e pinguedine eruti cognitionem valere mihi videbantur; ea ſpe fretus, fore, ut hujuſce diſquiſitionis chemicæ partem poſteriorem judicans non inferiorem priori, humaniſſime illam offeras meo nomine Sociis celeberrimis, in ſummæ meæ in illos reverentiæ documentum.

Propoſui ut noſti, vir celeberrime, modum iſtum, acidum pinguedinis concentratum acidi vitriolici ope obtinendi, quod nimirum, affuſum ſali ſegneriano ex hac partem acidam expellit forma vaporum. Quæ autem ne obnoxia ſit objectioni, quod huic vitrioli oleum ſit admixtum, et ut cognoſcerem, quomodo ſe habeat ſal medium noſtrum, igne uſtum, huic illud tradidi.

EXP. LVI.

Tres nimirum ſalis ſegneriani (ſeu ex noſtro, e pinguedine deſtillata eruto acido, et ſale alcalino vegetabili conflati) uncias, retortæ vitreæ loricatæ inditas, igni aperto per gradus aucto, expoſui. Deſtillabat in initio in excipulum aquoſi quid (cryſtallisationis nimirum aqua). Increſcente calore, ita ut retorta ignire inciperet, protinus ſurgebant vapores copioſiſſimi griſei, acidum forte, ut mihi quidem videbatur, præſagientes: ſed vaſibus frigefactis, et apertis nihil fumi percipiebam, nec odorem acidi ſuetum; ſed potius illum ſpiritus tartari, cui fluidum quoque obtentum (ponderis drachmarum XI.) in ceteris qualitatibus, e. g. ſapore, colore aureo, ſimile erat; cum ſale tartari, parum efferveſcens. Reſiduum ſal erat alcalinum, carbonacei modo quid continens; ſed alcalini volatilis ne veſtigium quidem prodens. Sicco jam præteribo pede, ſingularem acidi fortis (exp. 53.) VI ignis mutationem in mite, quæ etiam in terra foliata tartari, ſale acetoſellæ et tartaro ipſo obtinet, et a deſtructione quadam acidi, phlogiſto intime mixti pendere videtur, niſi forſan hoc acidum (ut cl. PRIESTLEY celeberr. collega tuus, illuſtris HUNTERE, in clyſſi nitri præparatione contendit) in aeris ſingularis ſpeciem tranſeat.

Modus acidum pinguedinis fumans obtinendi mihi huc uſque laboris et tædii plenus fuerat: peractis nimirum novem deſtillationibus (exp. I – 9.) et rectificatione (exp. 46.) acidum erat ſaturandum ſale alcalino, quod evaporandum, calcinandum, iterum ſolvendum et inſpiſſandum, priuſquam vitrioli oleum, purum ex eo acidum expelleret (exp. 53). Eundem nunc finem obtinendi methodo in compendium redacta, mihi in votis erat; quod, non ſine ſpe quadam ſequenti modo conficere tentabam.

EXP. LVII.

Impoſui veſicæ et alembico, cupreis, ſtanno intus obductis, ſebum depuratum, leni igne ſubſtrato nil niſi aquam emittens, quam autem, illo adaucto, ſequebatur fluidum viridescens. At eodem tempore ſtannum variis in alembici locis, præcipue in tubo huic appoſito, fundebatur et in externam ſuperficiem penetrabat. Finita deſtillatione, in excipulo inveni acidum et oleum, citra exſpectationem meam, utrumque fluidum, nec ut antea coagulatum, quamvis reſiduum totum fere in carbonem verſum eſſet. Hac quidem ratione laborum compendium quoddam repereram, a repetendis deſtillationibus et fuſionibus abſolutus; acidum vero non cupro ſolum inquinatum erat; ſed vaſa etiam ignis vi ita læſa erant, ut nonniſi magna adhibita opera, aliis laboribus inſervire iterum poſſent.

Spreta itaque hac methodo, votis non ex omni parte reſpondente, alia occurrit, periculum videlicet inſtituendi cum ſolutione ſebi in ſale alcalino, ſeu cum ſapone. Veriſimile enim mihi videbatur, illud dum ſolveret adipem, acidum præcipue, in hoc contentum, eſſe arrepturum: quo facto, ſi oleum ſaponis poſſet ſeparari a ſale ſegneriano, tunc ſtatim ad illud ſtadium proceſſus pervenirem, quod haud ſine mora, modo in exp. 46. eram aſſecutus: quæ autem ſeparatio facillima mihi videbatur, quia ſapo a quovis acido, nec non ſalibus mediis quibuſdam, in partes dirimitur: quo deſtructo itaque, oleum filtro ſeparare a fluido aquoſo, hoc evaporare, tunc addere vitrioli acidum, mens mihi erat. Hæc ponderans, percepi ſaponem communem non poſſe adhiberi, quia tum lixivium ex cineribus paratum varia ſalia media contineat, cum ſal culinare adhibeatur ad ſaponem ex aqua ſeparandum, quod pro parte huic ſe jungit. Saponis ita ſpeciem mihimet ipſi confeci.

EXP. LVIII.

Calcis vivæ recentis libram dimidiam itaque bene obrui cum ſalis tartari libra una, et linteo leviter tecta tam diu repoſui, donec calx findi et dehiſcere incipiebat. Tunc adfudi aquæ calidæ libras ſex, quæ coctæ in vaſe ferreo ad quartæ partis conſumtionem, per linteum denſum tranſcolui (quod autem lixivium nunc ovum recens ſuſtinebat). Quartam hujus partem, aqua tantiſper dilutam coxi cum ſebi libra una, donec maxima humiditatis parte evaporata, quam optime inter ſe coire inciperent. Affuſa nunc lixivii reliqua parte coxi leniſſimo igne, continuata agitatione, uſque dum mixtura pellucida et quaſi mucilaginoſa adparebat, et frigefacta gelatinæ inſtar concreſcebat, ſaponi communi, antequam ſal culinare adjicitur perfecte ſimilis. Separando nunc iterum oleo, a ſale alcalino nihil mihi magis idoneum videbatur, alumine, quia partim minimi conſtet, partim non timendum eſſet, illud via humida, quod ab acidis mineralibus expectare mihi fas fuiſſet, ſalem ſegnerianum eſſe deſtructurum, quo facto acidum pinguedinis in auram abiiſſet; aluminis vero acidum tantum modo terræ ſuæ ipſius actionem infringentis deponit, quantum ſalis alcalini liberi invenit.

EXP. LIX.

Gelatinæ itaque exp. anteced. in aqua ſolutæ injeci alumen pulveriſatum, quod eodem momento vi quadam oleum coactum in ſuperficiem urgebat. Hoc per cochlear cribratum ſublato, iterum alumen adjeci; atque eodem modo perrexi uſque dum poſt novam ejus additionem, nihil coacti ſuperficiem occuparet amplius[*]. Fluidum colatum (ut terra aluminis, et particulæ quædam olei coacti ſepararentur) ad ſiccitatem evaporavi[†].

EXP. LX.

Ut acidum, quod exſpectabam, ex ſale ſegneriano expellerem, alumen, adhibere mihi occurrebat, ut eo certius acidum illud a vitriolico liberum obtinerem. Quamobrem addebam 2. partibus ſalis 1. partem aluminis uſti, quas igni fortiori balnei arenæ exponebam. Peracta deſtillatione in excipulo inveni acidum fumans, ejuſdem naturæ ut illud exp. 53. eoque modo finem concentrandi proceſſus obtinuiſſe lætus perſpexi. Attamen illi acido animadverti odorem quemdam ſulphureum eſſe admixtum; et licet cl. BEAUMÉ (Chym. exp. t. 1. p. 335.) affirmet, parum acidi ex alumine expelli vi ignis ſolius; tamen a vero propius abeſſe in hoc proceſſu mihi videbatur partes oleoſas, maſſæ noſtræ ſalinæ adhuc adhærentes, ſeparationem acidi a terra aluminoſa promoviſſe. Qua re motus, potius oleum vitrioli adhibere decrevi, quia tum ignis gradum poſcat minorem, cum tota maſſa ſalina minor fiat.

EXP. LXI.

Tribus partibus maſſæ noſtræ ſalinæ[‡] affudi unam olei vitrioli, quod ſtatim vapores griſeos extricabat, acidum pinguedinis redolentes. Minor caloris gradus ſufficiebat omni expellendo acido: nam et maximus nihil amplius educebat præter quaſdam guttas olei ex bruno rubri *.

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[*] Reiteratis periculis hanc erui partium proportionem. Libris x. gelatinæ, in aqua ſolvendæ, addantur ſucceſſive aluminis unc. XXII. (quarum aqua cryſtalliſationis erit circa unc. XI. terra aluminoſa oz. IV. cum dimidia). Mixtio hæcce, colata, evaporata, dat ſalis unc. XXI. cum dimidia, quod ex tartaro vitriolato, ſale ſegneriano et aluminis haud deſtructi parte compoſitum eſt.

[†] Si fluidum illud ad cryſtalliſandum reponere placet, adhibito ſtudio, tartarus vitriolatus, et ſuperfluum alumen quoad maximam partem inde ſeparari, et quod reliquum eſt, inſpiſſari tunc poteſt: quo facto maſſa ſalina valde imminuitur.

[‡] Optima proportio hæc eſt: ſalis noſtri (exp. LIX. not. *) ¾ adduntur, olei vitrioli unc. IV. cum dimidia; acidum tranſtillatum adjicitur reliquæ maſſæ ¼, ut hoc modo rectificetur. Tunc habebis circiter unc. V. acidi limpidi fumantis.

Ut cognoſcerem, an vitriolico acido noſtrum ſit inquinatum, partem ejus quandam ſuperfudi ſoluto ſaturni ſaccharo, qua metallum exturbatum non iterum ſolvebatur adjecto vini aceto, quamvis illud digeſtione et ipſa coctione ſedimenti tentaverim †. Detecto itaque acido vitriolico; ſeparandum illud eſſe ab acido noſtro putavi, ſi nimirum, novæ maſſæ ſalinæ adjectum, iterum evocaretur: qua videlicet methodo acidum vitriolicum ſali alcalino nubens, pinguedinis acidum expelleret ‡.

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* Notatu dignum eſt, quod quavis via poſt evocatum acidum obtinerem aliquid ſalis ammoniaci animalis ſicci, quod ſcilicet in aqua ſolutum ſenſum frigoris excitabat, cum calce viva tritum odorem ſpargebat alcali volatilis, et cum acido vitrioli, illum acidi pinguedinis. Quod vero ſal alcalinum volatile, cum non ineſſe videbatur adipi, ut lateret in ſale tartari (cf. cl. WIEGLEB. de ſalibus alcalinis) vel ut hoc, ope olei, volatile ſit redditum (de quo autem dubito) neceſſe eſt.

† Cl. RETZIUS in Act. Acad. Stockh. t. 32. p. 216. contendit, certum hoc eſſe indicium abſentis acidi vitrioli, ſi ſedimentum ex ſaturni ſaccharo ſoluto per adjectum acidum ortum, reſolveretur addito nitri acido; ſed contrarium expertus ſum. Adjectis enim drachmæ I. acidi noſtri, guttis olei vitrioli quatuor, ſedimentum quidem in ſaturni ſaccharo ſoluto oriebatur; non ſtatim ſolvendum per nitri ſpiritum, ſed decantato a ſedimento fluido, et adjecta nova nitroſi acido portione, hoc utique diſparebat. Quod ſi vero eodem menſtruo plumbum præcipitabatur, hoc per unc. IV. aceti vini nullo modo rurfus diſſolvere valebam, quamvis drachm. V—VI. ſufficerent diſſolvendo ſedimento, abſque guttis IV. acidi vitriolici exorto.

‡ Hac methodo in uſum vocata, etiam uti licet cineribus clavellatis ad conficiendum ſaponem noſtrum; nam rectificando acidum noſtrum ſuper nova maſſæ ſalinæ quantitate, quodlibet acidum minerale illo pinguedinis admixtum, in illa maſſa remanet.

EXP. LXII.

Acidi itaque noſtri unc. IV. novæ maſſæ ſalinæ unc. I. adjectas leni igni expoſui; quo facto in excipulum tranſtillabat acidum fumans, coloris limpidi, quod ſaturni ſaccharo ſoluto admixtum ſedimentum quidem producebat, ad reſolvendum adjecto vini aceto.

EXP. LXIII.

Concentrati acidi noſtri vim in metalla experiundi nunc animus erat; illudque applicandi auro, quamvis ſpei haud multum rationes theoreticæ injicerent, fore ut ſolveretur. Auri itaque, ferri vitriolo præcipitati, granis IV. affudi acidi unc. I. quod leni calore digeſtum limpidum colorem in aureum mutaverat; magna licet metalli quantitate in fundo adhuc remanente; cujus vero coloris rationem peregrino cuidam, auro forte adhærenti, potius tribuens; idem cum hujus foliolis periclitatus ſum; ſed pari eventu. Quam itaque ſolutionem apparentem ut promoverem majore calore adhibito, acidum cum foliolis retortæ parvæ indidi aliamque implevi cum eodem acido atque granis IV. platinæ, et ex utraque fluidum, coquendo, evocavi, quod rurſus reſiduis affudi et digeſſi. Color in utroque fluido aureus erat, metallorum majori quantitate licet in fundo remanente. Phænomeni haud exſpectati novitate perculſus, atque de aliis metallis ſolvendis cogitans, foliola argenti noſtro acido immiſi; quæ vero, cum diſcerperentur, fluidumque aureum colorem indueret, non potui, quin ex hoc colore, ſolutioni argenti minime convenienti in eam inciderem opinionem, illum a ſolo ipſo acido pendere.

E X P. LXIV — LXXIV.

Unam itaque acidi limpidi unciam coquendo deſtillavi ad dimidiam; quo facto reſiduum aureo conſpicuum erat colore; finita vero deſtillatione, fluidum in excipulo tranſparens erat, in retortæ fundo vero animadvertebam circulos brunos concentricos. Fluidum ex excipulo infundebam novæ retortæ puræ, iterumque deſtillabam ad ſiccitatem, remanente pari materiæ brunæ quantitate. Eandem operationem repetii octies eodem modo; et ultima vice eandem, quam prima, repperi reſidui quantitatem; quod vero perfecte ſiccatum, in aqua plane non et in ipſo ejus acido, modo difficulter ſolvebatur; quin nitroſo acido, niſi calore adjuto, haud cederet. Acidum noſtrum fumandi vim amittebat; ejus vero acredo minime quavis deſtillatione ſic decreſcebat, ut differentia ſenſibus percipi poſſet; quæ autem ſatis conſpicua erat, ſi quod prima vice deſtillaverat, cum fluido quartæ deſtillationis, vel hæcce cum octava compareretur.

Notatu dignum utique eſt, quod hoc acidum diſtillando, vel digerendo colorem ſuum mutet, et quod antea totum volatile erat, nunc ſedimentum dimittat, et acredinis vim perdat, ita ut a vero propius abeſſe videatur, quod pertinaciori adhibita opera tandem penitus deſtruatur: qua itaque ratione medium eſſe noſtrum acidum cenſendum eſt inter acida mineralia, acetumque, eaque acida, quæ, ut tartarus et acetoſellæ ſal, ſine integra virium jactura plane tranſtillari nequeunt.

Convictus hiſce rationibus de fallaci auri in acido ſoluti augurio ex colore aureo; alia inſtitui hac de re experimenta.

E X P. LXXV.

Auri puri bracteolam, et platinæ granula quædam in vaſis bene occluſis per 6. hebdomadas calori fornacis expoſui: fluidumque tunc decantavi, ut adjecto ſale tartari viderem, an pars quædam ex illo præcipitaretur *, quod quidem minime eveniebat. Ex hac vero mixtione calori iterum expoſita, pulvis deſcendebat, qui, fluido decantato, aqua edulcoratus et ſiccatus albi coloris erat †. Quæ quidem terræ ſpecies hoc modo non ſolum non efferveſcebat: ſed etiam digeſtione adhibita difficillime ſolvebatur; alia et contraria ratione intermiſſa nimirum ſiccatione ſe habebat; cui ſolutioni, an quid metallici inſit, ut explorarem, tincturam ſulphuris volatilem beguini (metallorum optimum proditorem) adjeci; ſed præcipitatum ſulphur ejuſdem erat coloris, ut cum puro acido factum.

Hiſce obſervationibus inductus pulverem illum terram meram eſſe opinor, quam acidum noſtrum ſecum attollebat, quæque terrarum alcalinarum communium qualitatibus haud induta, ob volatilem naturam ad illam fluoris mineralis pertinere videtur; quod vero aſſertum experimentis probare, parca ſedimenti quantitas vetuit.

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* Mirum forſan poſſet videri, quare ſolutionem ſtanni in aqua regia non adjeciſſem: negare quoque non poſſum, me ab illa in periculo quodam adhibita, rubri coloris veſtigium obſervaſſe: ſed ab eadem ſtatim abſtinui, cum caſu viderem, fluida ſupernatantia præcipitationibus (e ſolutionibus plumbi ſtanni, reguli antimonii, biſmuthi et mercurii per acidum noſtrum factis) confuſa, invicem, novum præbere ſedimentum, rubello colore conſpicuum, cujus cauſa, ut infra patebit, in ſtanno latere videtur.

† Idem fere phænomenon obſervavi, miſcendo et digerendo ſolutionem alcalinam, cum pinguedinis acido, quod cum argento et biſmutho antea digeſſeram.

EXP. LXXVI.

Auri nunc calcis per ſal tartari paratæ gr. VIII. cum acidi noſtri unc. dimidia per menſis ſpatium digeſſeram, cujus tamen magna adhuc pars in fundo vaſis remanebat. Fluido colato addidi tincturam ſulphuris volatilem, quo facto mixtum colorem e cœruleo griſeum adeptum erat. Subſidentia facta, colatoque tunc fluido, reſiduum in filtro ſiccatum e nigro flavum erat, auri ſoluti præſentiam ſic demonſtrans; quod autem luculentius adhuc apparebat, evaporata parte quadam illius ſolutionis, e qua tunc cryſtalli e flavo brunæ, figuræ incertæ prodibant.

EXP. LXXVII.

Difficultatem ſolvendi auri vincere cogitabam addendo alia acida. Pari itaque aureæ calcis portioni, affundebam acidi pinguedinis guttas 40. quibus in uno vaſe addebam guttas 20. acidi nitroſi puri, in altero tantundem ſpiritus ſalis. In priori vaſe ſtatim fere conſpiciebam bullulas aereas ſeſe extricantes, atque ſolutionis initium indicantes: poſterius nullam mutationem patiebatur. Utrumque poſtea leni calore fovi; ſed licet ſolutio in priori increſceret; tamen in poſteriori nullum ejus apparebat veſtigium. Utriuſque fluidi guttas 8. infundebam in duas ſtanni ſoluti atque diluti portiones, quarum prior purpuram ſtatim dimittebat, poſterior, mutato haud colore turbidum modo aliquatenus evadebat.

EXP. LXXVIII.

Quo ex periculo cum ſpem haurirem, fore ut aurum metallicum ipſum ſolverem, ejus bracteolæ ſuperfudi guttas 80. acidi pinguedinis, et guttas 20. acidi nitroſi puri. Eodem fere momento tota ejus ſuperficies bullulis aereis tecta, placidaque erat illius ſolutio; additis vero adhuc acidi nitroſi guttis 20.; hæcce magis, magiſque increſcebat, donec, calore adjuta, totam bracteolam conſumeret. Quod quidem phænomenon argumentum eſſe poteſt diſcriminis, acidum noſtrum inter et illud ſalis intercedentis: certo enim certius eſſe videtur, duas partes acidi ſalis fumantis, et unam aquæfortis, aurum non poſſe diſſolvere; imprimis ſi digeſtio non adhibeatur: quam ob rem itaque pinguedinis acidum ſuo jure inter efficaciora acida locum ſibi vindicare videtur.

EXP. LXXIX.

Platinæ calcem ex aqua regis per tartari ſal præcipitatam, eodem modo (exp. 76.) tractavi, cujus ſolutio colata cum beguini tinctura ſedimentum deponebat obſcurioris coloris, quod in filtro collectum, ſiccatum ex flavo brunum erat. Solutionis altera pars evaporata in cryſtallos oblongas ex flavo brunas concreſcebat; quarum copia illa, ex auro obtentas multum ſuperabat.

EXP. LXXX.

Argenti foliola ab acido quidem noſtro corroſa, parum tamen ſolvebantur; paucæ interim ejus particulæ cupro immiſſo adhærebant; et ſalis acidum affuſum, aliqualem ſed vix conſpicuam præcipitationem producebat. Argenti vero calx continuata digeſtione ſolvebatur, ex quo, adjecta tinctura beguini, metallum ſulphuri adhærens fundum petebat, quod in filtro collectum, et ſiccatum nigreſcebat. Solutio evaporata in cryſtallos coibat, albo colore (ut argentum nitratum) haud conſpicuas; id quod acido longa digeſtione obſcuriorem colorem induenti tribuo, oleum vitrioli (minime vero ſalis ſpiritus) ſolutioni admixtum, ſedimenti quid procreabat.

E X P. LXXXI.

In mercurium noſtrum quidem acidum haud agere videbatur; ſed hoc ab illo altera vice abſtrahendo, obſervavi, metallum in pauco fluido reſiduo, mobilitatem ſuam ſolitumque ſplendorem perdidiſſe, et in maſſam quaſi cylindraceam, circumagendo vitrum, coire. Quæ quidem omnia fluido tranſtillato iterum affuſo adhuc perſtabant; admoto vero digeſtionis calore rurſus evaneſcebant. Poſtquam omne acidum ad ſiccitatem abſtractum erat, ſuperficiei retortæ majorem partem velut amalgamate obductam deprehendi, quod non a mercurii globulis, ſed a ſolidis quaſi argenteis pendere videbatur, quæ parti abſtractæ iterum affuſæ pro tempore innatabant, tandem vero ſubſidentes, ſolvebantur; id quod plus una vice obſervavi; colata ſolutio cupream laminam dealbabat; ſed illa ab adjecto ſale communi non turbabatur; mixtum vero hoc fluidum, colatum, cupro adhuc argenteum induebat colorem.

E X P. LXXXII.

Facilius adhuc calx (a mercurio ſublimato cum tartari ſale remixto, exorta) ab acido noſtro ſuſcipiebatur abſque caloris adjumento. Quod vero mixtum (exp. 81. motus) deſtillare decrevi in arenæ balneo; cujus ſub initium fluidi aliquid tranſibat, poſtea vero paululum adaucto calore, pars quædam ſublimata alba collo retortæ adhærebat. Hæc autem nova mercurii ſublimati ſpecies, aquæ deinde immiſſa, difficilius, adhibita etiam digeſtione, ſolvebatur, et adjecto ſale tartari ſedimentum album deponebat. Cum beguini tinctura commixta ſtatim in ſubſtantiam nigram, paulo poſt in cinnabarim mutabatur; cupro attrita, exacte licet ſicca, albam ei inducebat ſuperficiem; (id quod mercurius quoque ſublimatus communis præſtat). Noſtrum itaque acidum, excepto ſalis acido, ſolum omnium eſt quod ſublimatum ſiccum cum mercurio facit, atque (quod ſingulare utique eſt) leniori adhuc caloris gradu, ſurgit. Arena enim inferius circumdata retorta incumbebat, receptaculo ex tenui ferri lamina confecto, atque lateribus coctis ſuperpoſito: quo modo ignis vis et ob deficientem craticulam, et ob parvum foci ſpatium magna haud eſſe poterat.

EXP. LXXXIII.

Cuprum abſque prævia digeſtione ſolvebatur, teſte viridi fluidi colore; quæ vero adhibita, illud promovebat. Evaporando quidem cryſtalli apparebant, in aere autem mox deliqueſcentes.

EXP. LXXXIV.

Ferri facilis ſolutio ſaporis erat adſtringentis; in cryſtallos coibat aciculares, humiditatem atmoſphæricam vix attrahentes.

EXP. LXXXV.

Plumbum difficilius ſolvitur, et potius modo corroditur, minium vero acidum facile ſubit, quod jam, ante plenariam ſolutionem, rubrum colorem exuit; albo tunc pulveri ſimile. In ſolutionis (quæ a ſale culinari haud mutatur) ſaturatæ ſuperficie oriuntur cryſtalli ad $2'''$ fere longæ, pugiunculi forma præditæ, tandem deſidentes; quarum ſapor dulcis quid habet.

EXP. LXXXVI.

Regulus antimonii, abſtrahendo acidum ab illo, ſolvitur: ſi vero fluidum in excipulo illi in retorta adhuc reſiduo, affunditur, lacteum aſſumit colorem, nec pelluciditatem niſi adhibita digeſtione recuperat. Evaporata ſolutio in cryſtallos abit, in aere haud deliqueſcentes.

E X P. LXXXVII.

Solutio zinci facili negotio peragitur, quæ ſingulari ſapore metallico prædita eſt, et adjecto ſale tartari ſedimentum album deponit, quod flammæ admotum (ut zinci flores) flavum evadit.

E X P. LXXXVIII.

Stanni Malaccenſis raſura ab acido noſtro in pulverem flaveſcentem corrodebatur; et majori quidem adhuc vehementia, ſi calori exponeretur, ſic ut hujus unc. dimidia deſtruendis illius ſcrup. II. ſufficeret. Odor ex mixtura ſurgens maxime ingratus, et illi fere ſimilis erat, quem ſalis acidum cum zinco edit. Fluidum paucum ſupernatans turbidum erat, quod omni ſtudio decantatum chartæ bibulæ ſuperfudi; ſed haud mutatum tranſibat: quin chartam iſtam duplicatam, immo quadruplicatam penetraret; haud minus, quam antea turbulentum. Aliquo tempore poſt pulvis ſubſidebat flaveſcens, atque ei ſupernatans, fluidum pellucidum colore pulchre roſeo ſplendebat; quod decantare fruſtra tentabam; ſimulac enim vitrum ſolum modo tangebam, ſumma imis miſcebantur; et fluidum chartam bibulam, eadem ſub ſpecie turbida penetrabat; poſtea iterum ſubſidens. Cui vero colori rubro ſedimentum rubellum (exp. 75. n. *) tribuendum videtur.

Hanc corroſam calcem ſtanneam digerebam aqua deſtillata, quæ colata dein atque evaporata relinquebat ſal album facile deliqueſcens: quod ſi vero eidem calci affunderem novam acidi copiam, ut eandem ex toto in fluidum roſeum liquerem, illa quidem mox colore iſto ornata, ſed ſedimenti quantitas haud imminuta erat; quæ nunc calori expoſita, illud non ſolum non ſolvebat, ſed et gratum colorem cum flavo commutabat.

EXP. LXXXIX.

Biſmuthum, per longum quidem temporis ſpatium cum acido licet digeſtum, non ſolvebatur; contrarium vero eveniebat adhibita calce (quam ex ſolutione illius cum nitroſo acido multa aqua diluto facta) dejecerat admixtum ſal alcalinum. Quæ vero ſolutio ab aqua adjecta lacteſcebat; et ſedimentum album dimittebat; ab acidis vero vitrioli et ſalis mutationem nullam experiebatur.

EXP. XC.

Cobalti ex ſmalta reductus regulus, eodem ut biſmuthum modo digeſtus, eadem pertinacia acido reſiſtebat: calx vero, ex nitrato cobalto per tartari ſal deſidens, eidem facile cedebat *. Cujus ſolutionis drach. tribus cum unam nitri adjicerem, et deſtillationem inſtituerem, hac ad medium perducta vapores percepi flavos, verſus illius finem rubros (exp. 117). Partem ſalis jam cryſtalliſati et viridis diſtincte obſervabam dealbari a vaporibus nitroſis; quod deinde ſolutum in aqua, ſpeciem conſtituebat atramenti ſic dicti ſympathetici, ex flavo virideſcentis.

EXP. XCI.

In regulum niccoli (repetita ſæpius, etiam adjecto carbonum pulvere, toſtione, eique interpoſita fuſione cum nitro, calce, et borace paratam) nulla fere erat acidi noſtri etiam cum illo digeſti actio. Adjectum ei ſal tartari nihil deturbabat, quum e con-

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* Hæcce ſolutio, calori expoſita, partem aliquam dimittit, quæ poſtea non iterum ſuſcipitur; id quod in aliis ſolutionibus e. g. niccoli et biſmuthi, quoque obſervavi.

trario tinctura beguini, parvam metalli quantitatem ſulphuri unitam dejiciebat. Calx e niccolo nitrato per alcalinum ſalem præcipitata abſque digeſtione diſſolvebatur ab acido noſtro, cujus color evadebat virideſcens, et adjectis acidis vitrioli et nitri nihil dimittebat.

E X P. XCII.

Arſenicum album magna cum difficultate, digeſtione licet adhibita, ſolvebatur ſic, ut ejus ſcrupulus unus ab acidi uncia dimidia vix ſuſciperetur. Caloris ope autem hanc ſubibat major illius copia, quam eo abſente ſuſtentari poterat; quo factum ut tunc parvæ cryſtalli fundum peterent. Si noſtræ ſolutioni immittebatur cuprum, illud nihil dejiciebat, ſed potius pro parte ab acido ſuſcipiebatur. Parte aquoſa ſenſim evaporante, ſal apparebat e viridi cœruleum; ſub finem vero aliud ſaturate viride; luculento argumento, prius eſſe compoſitum ex arſenici acido et cupro; alterum ex noſtro acore, eodemque metallo.

Altera pars ſolutionis adjecto ſale alcalino nihil dimittebat; ea quoque pars, cujus fluidum in auras abierat, adjecto oleo tartari per deliquium ex integro ſolvebatur; quod vero non ita multo poſt ſedimentum deponebat, nova ſalis tartari portione non auferendum; idque ſal neutrum arſenicale fuiſſe, parca aquæ quantitate haud ſolvendum cenſeo.

E X P. XCIII.

Magneſii mineram Ilfeldenſem digerebam cum noſtro acido, quod illam in initio corrodebat, pulveremque nigrum a cryſtalliformi minera ſeparabat; deinceps vero illam in parca haud quantitate ſolvebat. Quod cum aliis metallis digeſtum acidum colorem brunum induebat, cum magneſio nullam patiebatur mutationem; et odorem ſpargebat ad illum ſolutionis ſtanneæ accedentem; ſaporis erat metallici. Addita aqua illam turbidam paululum reddebat. Vitriolicum acidum vero nihil dejiciebat (indicio, terram calcaream mineræ haud ineſſe) cum ſale fixo alcalino mixtum; ſedimentum copioſum dimittebat, quod ſtatim novo adjecto acido iterum diſparebat; ſi vero converſa via ſal alcalinum ſedimento majori adhuc copia ſuperfunditur, illud diſſolvit; adjecto vero tunc acido, e. g. ſalis, metallicam partem iterum deponit. Tinctura beguini acidæ noſtræ ſolutioni admixta colorem præ ſe ferebat rubellum, et quæ copioſiſſime dimittebat, ſiccata, ejuſdem adhuc erant coloris.

Abſolutis nunc iis, quæ de actione acidi noſtri in metalla dicenda erant, ſupereſt adhuc, ut videamus, quam rationem habeat idem acidum metallicis ſolutionibus adjectum.

Præcipitationes metallorum, in aliis acidis ſolutorum, per acidi pinguedinis admixtionem exortæ.

EXP. XCIV.

Aurum. Hujus metalli ſolutionem in aqua regis (quam vitro, in quo medicamenta fluida aſſervari ſolent, infuderam) aeri libero expoſueram, ex qua hac ratione tandem pulchræ cryſtalli flavæ exortæ erant, quæ figuræ ſalis communis appropinquantes, ex ſuperimpoſitis lamellis angulatis conſtabant, in aere vero per plures hebdomadas detentæ haud diffluebant *. Quas quidem cryſtallos in aqua ſolvebam deſtillata ſimplici, quæ, adjecto noſtro acido, ſedimentum flavum dimittebant. Decantato fluido, illud lavabam nova aquæ quantitate, qua defuſa, cum alia ejuſdem deſtillatæ portione adjecto, ſedimentum per plures dies digeſſi, colavi, evaporavi, atque hoc modo obtinui reſiduum, aquam ex aere attrahens.

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* Aliquo tempore poſt inveniebam haſce cryſtallos motum velut inteſtinum aſſas, volumine adjecto in farinoſam vel flocculentam-ſubſtantiam mutatas.

E X P. XCV.

Platina. Ex ejus ſolutione in aqua regis acidum noſtrum dejiciebat pulverem fere aurantium, qui, edulcoratus, multa aqua ſuperfuſus, digeſtus, colato atque evaporato fluido, reſiduum exhibebat e griſeo flavidum, quod in aere auro minus diffluebat.

E X P. XCVI.

Argentum. Quod acidum pinguedinis ex argento nitrato præcipitabat *, coloris erat griſei paululum in rubellum vergentis; et, edulcoratione prægreſſa cum aqua digerebatur. Cujus partiuni addebam guttas aliquot acidi vitriolici, a quo imperfecta oriebatur præcipitatio: altera, evaporata, relinquebat reſiduum aquam valde attrahens: argentum vitriolatum ſtatim adjecto acido ſedimentum album dimittebat; luna vero cornua cum acido digeſta haud mutata videbatur.

E X P. XCVII.

Mercurius. Acidum noſtrum album producebat ſedimentum ex mercurio nitrato †. Sed (quod notatu quam maxime dignum mihi videtur) idem ex ſublimato aliquid exturbabat ‡: affuſo enim illo, mixtio paulo poſt lactea evadebat; pulverem

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* Promptior adhuc erat præcipitatio per ſal ammoniacum animale.

† Hic quoque magis adhuc præcipitabatur adjecto ſale ammoniaco animali.

‡ Mercurius, acido ſalis optime nuptus, addito acido vitriolico minime mutatur; atque ſi aquæ, ſale ſelenitico fœtæ, ſublimatum ſolventes, aliquid flavi deponant, (cf. cl. BEAUMÉ Chym. t. II. p. 434.) quod et ipſe vidi, tribuendum mihi hoc videtur affinitati duplici, quum nimirum acidum ſalis terram calcaream quoque amet, eamque ob rationem metallum vitriolico acido cedat.

dein

dein album deponens; quod eo citius evenit, ſi mixtum digeritur. Fallor; aut hoc ſedimentum album ex ſublimato corroſivo hac ratione ortum criterii inſtar eſſe poteſt ad diſtinguendum acidum noſtrum ab aliis, præcipue vero a muriatico. Illud vero ſedimentum ablutum, digeſtum, in aqua ſolvebatur, eique immiſſum cuprum albeſcebat; eadem quoque ſolutio evaporata reſiduum dabat album, in aere non liqueſcens.

EXP. XCVIII.

Plumbum. Quæ e ſaturno nitrato deſcendebant cryſtalli parvæ aciculæ formam gerentes, edulcoratæ, aqua digeſtæ facilius ſolvebantur; acidoque tunc vitriolico admixto ſedimentum dimittebant. Diſſipata illius ſolutionis humiditate ſuperſtes erat pulvis aquam parum attrahens.

EXP. XCIX.

Biſmuthum. Nitroſum acidum, metallo ope digeſtionis ſolvendo deſtinatum, tanta aquæ copia dilutum erat, ut, ſolutione peracta, nova illi admixta aqua nihil præcipitaret. Simulac vero acidi noſtri guttæ aliquot accedebant, pulverem album dejiciebant, qui ablutus, cum aqua digeſtus, ſolutione colata, evaporata, reſiduum album progignebat facillime liqueſcens.

EXP. C.

Regulus antimonii. Saturata ejus in aqua regis ſolutio, addita aqua deſtillata, turbida evadebat; quam colatam, nova adjecta aqua non amplius mutabat: ab affuſo vero noſtro acido, ſtatim oriebatur ſedimentum album, ex quo aqua extrahebat partem fluido diſſipato conſpicuam, quæ, humiditatem iterum attrahens, in cryſtallos parvas tenues coibat.

E X P. CI.

Stannum. In aqua regis ſolutum metallum dejiciebatur ab acido noſtro, et colorem induebat ex flavo brunum. Præcipitatum ablutum, et cum aqua digeſtum ſal progignebat albidum facillime liqueſcens *.

E X P. CII.

Cuprum. Hoc neque ex vitrioſi cœruleo, nec ex cupro nitrato per pinguedinem præcipitabatur.

E X P. CIII.

Ferrum, nec nitratum, nec vitriolatum, cum acido noſtro mixta ſedimentum dimittebant.

E X P. CIV.

Zincum, nec nitratum, nec vitriolatum mutabantur ab acido noſtro adjecto.

E X P. CV.

Cobalti regulus, nitratus ab acido pinguedinis affuſo nullam perturbationem paſſus eſt.

E X P. CVI.

E niccoli regulo nec nitrato, nec ſalito acidum noſtrum aliquid extricare valebat.

E X P. CVII.

Arſenicum nitratum, acido noſtro admixtum, nullum ſedimentum deponebat.

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* Hæc quæ ab acido noſtro natales ducunt præcipitata, nil eſſe videntur quam ſalia metallica in aqua ſolutu difficilia.

E X P. CVIII.

Magneſium nitratum nullam converſionem expertum eſt a noſtri acidi admixtione.

*Acidorum diverſorum actio in ſal ſegnerianum**.

Quod *vitriolicum* acidum, noſtrum acorem ex ſale hoc medio expellat; jam ſupra protuli.

E X P. CIX.

Nitroſum acidum. Duabus ſalis noſtri drachmis affudo tantundem aquæ fortis, ut vocant, duplicis (quam peracta ejus præcipitatione deſtillaveram) nulla efferveſcentia ſenſibili inde oriente. Quod peracta deſtillatione excipulo inerat fluidum, ſaporis acidi noſtri proprii erat, ſed odori admixtum aliquid aquæ fortis. Decompoſitum vero fuiſſe ſal noſtrum, ejuſque acidum expulſum, demonſtrat præcipitatio celerrima e ſaturno nitrato, per fluidum illud, quod deſtillando obtinueram.

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* De figura hujus ſalis monenda quædam adhuc mihi ſuperſunt. Quod ad terram foliatam accedat (Exp. nov. p. 2.) aſſerui, Segnerum ſecutus, ſed cum illud majori in copia paraviſſem, et maſſam ſalinam perſcrutarem, inveni illam cruſta firma ſuperius tectam, qua vero ablata, huic adhærebant multæ cryſtalli, tres ut plurimum lineas longæ, pugionis quadrangularis forma conſpicuæ, quarum duo latera oppoſita ceteris anguſtiora erant. Si ſalis alcalini quantitas nimia haud adjecta eſt, et cryſtalli ſiccantur ſuper charta bibula; illæ in aere haud diffluunt: qua ratione, ut et cryſtallorum forma, a terra foliata tartari, mirum, quantum differunt. Segnerus vero in hanc opinionem ex eo deductus videtur, quod ſalis medii parcam modo quantitatem pararet, quapropter latentes ſub cruſta ſalina cryſtallos, ob ſpatii et materiæ defectum, obſervare haud poſſet. Forſan nec acidum ejus ab oleoſis particulis ſatis liberum; nec ad ſaturationem aliud ſal alcalinum, quam cineres clavellati, adhibitum erat.

EXP. CX.

Muriaticum acidum. Æquale noſtri ſalis medii et acidi muriatici pondus commiſcebam: quæ deſtillata, exhibebant acidi pinguedinis drachm. II. proprio odore præditas, et e ſublimato corroſivo pulverem album præcipitantes.

EXP. CXI.

Aceti vini optimi drachm. VI. ſuperfudi ſalis noſtri drachm. II. quod ex hiſce deſtillando obtinui fluidum odoris erat aceti, et, ſublimato corroſivo admixtum, illud intactum relinquebat. Cum in majorem rei explanationem reſiduo in retorta adjicerem ſalis ſpiritum, et deſtillarem, acidum, adipis ſe prodebat jam odore, et præcipitatione mercurii ſublimati.

EXP. CXII.

Fluoris acidum pari pondere noſtro ſali admixtum celerrime in illud penetrabat, ut ſiccum videretur. Quod nonniſi magno caloris gradu adhibito prodibat fluidum, fluoris acidum haud mutatum erat: quod etiam ſaturno nitrato affuſum ejus pelluciditatem (ut ei moris eſt*) non tollebat; hac ratione quam maxime abhorrens ab acido pinguedinis.

EXP. CXIII.

Phoſphori ſal. Hujus in aqua ſoluti unc. dimidiam addebam ſalis medii noſtri drachm. II. Sub initium deſtillationis fluidi quid tranſibat, quod vero nil niſi aqua erat. Qua ex excipulo evacuata, ignem adaugebam † quo adhuc aliquid ex maſſa extorquebam, quod vero nec acidum erat, nec ſaturni ſaccharum decomponebat.

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* Cf. cl. SCHEELE in Comment. Stockh. vol. XXXIII.

† Idem erat hujus gradus, qui ad ſublimandum ſal ammoniacum animale requiritur, et majorem adhuc adhibere dubitabam, cum ſal noſtrum ſolo igne vehementiori jam decomponatur.

EXP.

EXP. CXIV.

Arſenici albi et ſalis noſtri paullìſper flavidi parem quantitatem in pulverem album comminuebam, quorum actionem in ſe reciprocam ut promoverem, adjiciebam aquæ deſtillatæ drachm. II. quæ omnia leni calore digerebam. Elapſa vix horæ una quarta, pars quædam pulveris nigreſcebat, et parieti in forma annuli nigri * fortiter adhærebat, reliqua maſſa ſalina ab illo ſeparata erat. Deſtillando (eodem, ut in exp. præcedenti, ignis gradu) parum fluidi obtinui, quod nec ſapidum erat, nec plumbum ex ejus ſaccharo præcipitabat. In collo retortæ parum ſublimati, ejuſque tenuis reperi.

EXP. CXV.

Cobaltum nitratum. Salis noſtri drachm. unam immiſi ſolutionis cobalti nitratæ unc. dimidiæ, et fluidum penitus evocavi. Sal exſiccatum in retorta viridi gaudebat colore, quod frigefactum hunc commutabat cum albo: illud ſolvebam in aqua deſtillata, quæ nunc exhibebat novam atramenti ſympathetici ſpeciem, cobaltino communi haud abſimilem, in luteum modo magis vergentem colorem.

EXP. CXVI.

Salis ammoniaci animalis (ex acido pinguedinis et alcali volatili compoſiti) drachm. II. commiſcebam cum lapidis ſic dicti hæmatitæ granis XV. quæ igni expoſita ſublimatum exhibebant; quod vero, peracta operatione immutatum eſſe ſal ammoniacum, reperiebam, relicto in fundo hæmatite. Eadem iterum miſcebam, addendo aquæ aliquid in meliorem utriuſque combinationem: ſed tamen eadem eveniebant omnia.

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* Nil hoc mihi videtur, niſi ſubita reductio quædam arſenici albi in regulum, deſtillatione quoque peracta nigrum adhuc erat et durum; quod abraſum, detegebat maſſam albam, nonnihil firmam. A vero proprius mihi eſſe videtur, reductionem hanc ortam eſſe phlogiſto, ſali flavido inhærente.

Actio acidi pinguedinis in ſalia media.

EXP. CXVII.

Nitrum. Hujus exacte depurati drachmis II. ſuperfudi acidi noſtri tantundem, quod illud cum aliqua vehementia ſolvebat. Retorta vix arenæ calidæ immiſſa, flavida jam a vaporibus evadere mihi videbatur: qui color ſemper ſaturatior fiebat, donec eundem ruborem acquireret, qui adhibito vitriolico acido conſpicitur. Fluidum in excipulo odore acido, nitroſo communi, præditum erat, cui etiam aliquid acidi pinguedinis admixtum; nam argentum purum non ſolito ab aqua forti more ſolvebatur; potius cruſta tegebatur ſatis craſſa, colore dilute hepatico conſpicua.

EXP. CXVIII.

Sal muriaticum. Hujus drachm. II. ſolvebam in acidi noſtri pari pondere. Deſtillatione ad finem tendente, diſtincte obſervabam vapores griſeos: odor fluidi in excipulo contenti, erat acidi muriatici: ſed ut hoc cum certitudine quadam conſtaret, et an acidum pinguedinis admixtum ſit, exploraretur, res haud parum perplexa erat, cum utrumque magnam inter ſe alat ſimilitudinem. Cui vero fini reſpondere ſtannum poſſe judicabam; eamque ob cauſſam miſcebam, 1. aquæ fortis guttas 80 cum guttis 40 ſpiritus ſalis; 2. eandem aquæ fortis et ſpiritus ſalis copiam (ut 1.) cum acidi pinguedinis guttis 40; 3. aquæ fortis guttas 80 cum acidi pinguedinis guttis 40. Unicuique harum mixtionum deſtinabam ſtanni malaccenſis ſcrup. II. et in quodvis vitrum, a quavis ſtanni portione, tenuiſſima fila non prius immittebam, quam olim injecta, abſque caloris auxilio, plane diſparuiſſent. Quævis enim mixtiones in metallum agebant: N. 1. maxime, N. 3. minus, N. 2. minime: cum vero N. 1. ſolvendo ulterius ſtanno impar eſſet, grana hujus 7. adhuc ſupererant: ſolutio erat pellucida, et abſque ullo ſedimento. N. 2. maxime turbidum erat, coloris e griſeo flavidi; ſedimento copioſo nigreſcente refertum; ſtanni reſidui pondus erat gran. XVII. N. 3. exhibebat ſolutionem pellucidam, cum parvo in brunum vergente ſedimento; ſtanni granis 9 adhuc ſuperſtitibus. Hiſce periculis peractis, quæ regulæ inſtar eſſe debere mihi animus erat, admiſcebam guttis 80 fluidi ex deſtillatione obtenti guttas 160 ejuſdem aquæ fortis. In qua mixtione diſſolvebam peditentim (iiſdem phænomenis apparentibus, ut in N. 1.) ſtanni fila, donec drachma 1. conſumta eſſet; ſed ſedimenti nigri quid in fundo hærebat. Quibus ponderatis, concludendum mihi eſſe videtur, quod fluidum in excipulo repertum acidum fuerit muriaticum; id quod præprimis ex vaporibus griſeis, et ex magna ſtanni copia, quæ abſque magna ſedimenti copia ſolvebatur, colligo, pinguedinis vero acidum haud admixtum fuiſſe; ea ex ratione cenſeo, quia ſolutio clara, nec ſedimentum brunum erat; nigrum vero illud ex eo ortum fuiſſe judico, quod ſalis ſpiritum (concentratiorem, quam opinabar) non diluerem nitroſi acidi quantitate ſufficiente.

EXP. CXIX.

Terra tartari foliata. Huic addebam æquale pondus acidi noſtri, cum qua paululum efferveſcebat; peractaque tunc deſtillatione acetum excipulo ineſſe prodebat odor, nec non deficiens in ſublimatum corroſivum actio.

EXP. CXX.

Sal mirabile Glauberi. Quamvis haud expectari poſſe videbatur acidum noſtrum expulſurum eſſe vitriolicum, tamen experientiam conſului. Utroque æquali pondere mixto et deſtillato, inveniebam in excipulo fluidum, quod præter odorem acidi noſtri, ſulphureum quoque admixtum habebat. Quamobrem illud affudi ſolutioni plumbi in acido noſtro factæ, quæ ſedimenti albi quid dimittebat; indicio, parvam acidi vitriolici quantitatem divulſam eſſe a ſale alcalino; quod phlogiſto acido adhuc adhærenti tribuo, quo nimirum vitriolici acidi pars volatilior reddita videtur.

EXP. CXXI.

Tartarus tartariſatus in aqua ſolutus, adjecto acido ſedimenti magni copiam dimittebat, quod fluido decantato, veri cremoris qualitates demonſtrabat.

Liceat hiſce experimentis quædam addere de ſimilitudine et cognatione acidi noſtri et muriatici. Utrumque cnm alcali volatili conſtituit ſal ammoniacum ſiccum, et cum magneſia alba, ſal valde diffluens; utrumque argentum et mercurium e menſtruis præcipitat; ab utroque ſoluto regulo antimonii, adjecta aqua turbatur, et metallicam partem deponit. Eandem cognationem hoc quoque indicare videtur, quod acidum muriaticum ſolutionis argenti et mercurii in acido noſtro non præcipitet. Sed magna etiam inter utrumque intercedit differentia: noſtri nimirum acidi intima combinatio cum oleoſis partibus; ſal calcareum haud diffluens; naphthæ facilis geneſis; argenti et mercurii ſolutio via ſimplici humida; et hujus præcipitatio ex ſublimato corroſivo.

His inter ſe comparatis, characteres cognationis utriuſque acidi, quam illi diſcriminis, potiores mihi omnino eſſe videntur.

Hæc ſunt, illuſtris et celeberrime HUNTERE, pericula, quæ cum acido pinguedinis hucuſque inſtitui, omnia. Minime me fugit, materiam hanc nondum eſſe exhauſtam; multaque adhuc eſſe ſupplenda, quæ intimiori noſtri acidi cognitioni favent; imprimis explorandos eſſe affinitatis gradus, quos metalla contrahunt cum acido noſtro. Quibus vero omnibus exantlandis minime deero, ſi modo cognovero, labores meos huic rei impenſos plane haud diſplicuiſſe eruditis, imprimis illuſtriſſimæ Regiæ Societati, cujus aliquem in me favorem quam maxime cupio. Interim vale, vir celeberrime; meque tibi ut habeas commendatiſſimum, majorem in modum oro rogoque. Dabam Helmſtadii Idibus Decembr. 1780.

III. *Obſervations on the Bills of Mortality at* York. *By* William White, *M. D. F. A. S.; communicated by* Nathaniel Pigott, *Eſq. F. R. S.*

Read December 6, 1781.

FAITHFUL and accurate regiſters of the number of births and deaths kept in different places are of great importance to the community. The ſtateſman, the philoſopher, and the phyſician, are equally intereſted in inquiries which infallibly ſhew us the real ſtate of the nation, as to population, healthfulneſs, and, as connected with the latter, virtue and temperance.

It muſt give great pleaſure to a reflecting mind, to find, from undeniable proofs, that this nation appears to be, in the above reſpects, in a general and progreſſive ſtate of improvement. The births have become more numerous, the deaths fewer, in proportion in almoſt every place where the regiſters have been conſulted: for proof of this I refer to the Tranſactions of the Royal Society, vol. LVII. LIX. LXI. LXIV. LXV. &c. and to a publication of Mr. WALES, F. R. S. intituled, An Inquiry into the preſent State of Population in England and Wales, lately publiſhed.

It would not perhaps be difficult, and as a phyſician I could with pleaſure attempt the inveſtigation, to diſcover the various cauſes to which ſuch effects may be attributed; but here a wide field offers itſelf to our examination. It will, however, be neceſſary juſt to point out ſuch as affect this city in particular, in a ſubſequent part of this paper.

Mr. DRAKE, F. R. S. in his Antiquities of York, has given us the number of births and burials for 7 years, from Auguſt 5, 1728 to Auguſt 5, 1735, incluſive. This gave a favourable opportunity of comparing our preſent ſtate after an elapſe of 45 years. In order to this, the different pariſh regiſters were carefully examined from January 1, 1770, to December 31, 1776, incluſive: I added the number of males and females for the latter term, which Mr. DRAKE omitted.

TABLE I. The number of births and burials in York from Auguſt 5, 1728, to Auguſt 5, 1735.

The different pariſhes.	Births.	Burials.
All Saints, Pavement,	123	218
All Saints, North-ſtreet,	101	111
St. Crux,	132	159
St. Cuthbert's,	55	80
St. Dyonis,	92	106
St. Helen's,	113	122
St. John's,	136	173
St. Laurence,	60	77
Martin's, Conyngs-ſtreet,	73	110
Michael le Belfray,	310	327
St. Mary's, Caſtle-gate,	150	221
St. Michael, Spurrier-gate,	198	216
St. Martin's, Mickle-gate,	92	117
Biſhophill the elder,	103	117
Biſhophill the younger,	57	73
St. Maurice,	55	158
St. Margaret's	118	147
St. Olave's,	147	181
St. Saviour's,	70	103
St. Sampſon's,	188	228
Chriſt Church,	140	119
Trinity, Goodramgate,	143	144
Trinity, Mickle-gate,	129	152
Diſſenters,	18	29
	2803	3488

The burials, therefore, exceeded the births 685 in 7 years, or 98 annually.

TABLE II. The number of births and burials from January 1, 1770, to December 31, 1776, incluſive.

The different pariſhes.	Births.	Burials.
All Saints, Pavement,	240	153
All Saints, North-ſtreet,	96	88
St. Crux,	146	109
St. Cuthbert's,	102	126
St. Dyonis,	109	96
St. Helen's,	96	76
St. John's	183	124
St. Laurence,	97	83
Martin's, Conyng-ſtreet,	104	74
Michael le Belfray,	297	298
St. Mary's, Caſtle-gate,	159	210
St. Michael's, Spurrier-gate.	151	113
Martin's, Mickle-gate,	82	98
Biſhophill the elder,	124	151
Biſhophill the younger,	121	92
St. Maurice,	76	138
St. Margaret's,	182	142
St. Olave's,	234	296
St. Saviour's,	96	108
Sampſon's,	174	284
Chriſt Church,	147	110
Trinity, Goodram-gate,	161	118
Trinity, Mickle-gate,	122	164
Diſſenters,	24	24
	3323	3175

Decreaſed in burials 313, or $44\frac{5}{7}$ annually.
Births increaſed 520, or $74\frac{2}{7}$ ditto.
Births exceed the burials 148, or 21^{1}, ditto.

TABLE III. The number of births and burials, with the proportion of males and females, annually, from January 1, 1770, to December 31, 1776.

	Births.	Males.	Females.	Burials.	Males.	Females.
1770	467	237	230	417	203	214
1771	451	225	226	485	225	260
1772	490	238	252	508	220	288
1773	474	244	232	499	241	258
1774	453	214	239	382	173	209
1775	490	255	243	488	237	251
1776	498	255	243	396	177	219
	3323	1666	1657	3175	1476	1699

Number of males born in 7 years 1666, or 238 annually.
Number of males buried in 7 years 1476, or $210\frac{6}{7}$ annually.
Number of females born in 7 years 1657, or $236\frac{5}{7}$ annually.
Number of females buried in 7 years 1699, or $242\frac{5}{7}$ annually.

TABLE IV. Mortality of the ſeaſons.

Winter.		Spring.		Summer.		Autumn.	
Jan.	320	Apr.	277	July	220	Oct.	237
Feb.	282	May	265	Aug.	237	Nov.	230
Mar.	316	June	274	Sept.	225	Dec.	292
	918		816		682		759

In order to find the number of inhabitants in any place, where, either from its bulk, or other reaſons, a numerical ſurvey cannot be obtained, two methods may be made uſe of. The firſt is, multiplying the number of houſes by the medium of inhabitants in each. The ſecond is, one recommended by Monſ. MOHEAN, in a work, intituled, *Recherches et Conſidera-*

tions ſur la Population de la France. He found, by very laborious calculations, that the number of inhabitants may be known by the births, the latter being to the former as nearly 1 to 27.

By an account given into the Houſe of Commons in March 1781, the number of houſes in York ſubject to the new houſe-tax was 2285: if to thoſe be added ſuch as were too ſmall to come under the tax, which may probably amount to one-third more, the total of the houſes in York will be about 3000. This number multiplied by 4¼, which is nearly the medium of people in a houſe, gives 12,750 for the number of inhabitants.

By the ſecond rule we have 12,798 for the number of inhatants, which is the reſult of 474, the average annual births, multiplied by 27.

The remarkable coincidence of the above methods of calculation makes it very probable, that if we eſtimate the number of inhabitants at 12,800, we ſhall not be far from the truth.

However this may be as to the exact number of inhabitants, it affects not the principal end of the preſent inquiry, which is to ſhew how we are improved in population and healthfulneſs within 40 years paſt.

In order to prove this, we muſt find the number of inhabitants in the year 1735, from tab. 1. We there find the average annual births to be 400; this multiplied by 27 gives 10,800 for the number at that time. This number divided by the average annual deaths 498, gives the proportion of deaths 1 in 21¾. Such was the ſtate of this city as to mortality 46 years ago.

Very different from this is our preſent ſituation, the proportion of deaths being now decreaſed to 1 in 28¼, which is the quotient of 12,800, the number of inhabitants divided by 453, the preſent average of annual deaths. This is certainly a great riſe in the ſcale of healthineſs. From being near as fatal as London we have become leſs ſo than many country places, as will appear from the following comparative view of the proportion of deaths in different places.

At Vienna,	1 in 19½ dies every year
London,	1 in 20¾
Edinburgh,	1 in $20\frac{2}{3}$
Berlin,	1 in 21
Rome,	1 in 22
Amſterdam,	1 in 22
Dublin,	1 in 22
Leeds,	1 in 22
Northampton,	1 in 26
Shrewſbury,	1 in 26
Liverpool,	1 in $27\frac{7}{10}$
Mancheſter,	1 in 28
York,	1 in 28½

Hence in 1735, at York it would require 21¾ years to bury a number equal to that of its inhabitants; but in 1776, 28½ years would be required for the ſame. One third leſs die yearly now than in the former period; and we are certainly advancing ſtill higher, for in 1777 the births were more than in any former year, being 516, the burials 464.

As there is no ſettled manufactory here, there is little increaſe or decreaſe of the people by acquiſition or emigration, and probably what may happen in either caſe is nearly balanced by the other.

It appears from tab. 4. that the ſummer ſeaſon is by much the healthieſt at York; autumn the next; then the ſpring; winter being by far the moſt fatal. Dr. PERCIVAL found much the ſame to be the caſe at Mancheſter. At Cheſter Dr. HAYGARTH ſays November was the moſt ſickly month. The differences in the regiſters make it impoſſible to give the diſeaſes of which the individuals died; yet a general idea of this may be obtained from the ſame table. By the care and attention of the preſent archbiſhop of this province, this may be eaſily perfected in future periods.

It appears from hence, that our diſeaſes are chiefly of the inflammatory kind, which phyſicians know to be the general attendants of the winter and ſpring months. The diſorders of the ſummer and autumn are more particularly ſuch as ariſe from putreſcency and acrimony, ſuch as ſlow and remitting fevers, dyſenteries, cholera's, and the like, thoſe then being with us the healthieſt ſeaſons ſhew that we are not ſubject to putrid diſeaſes. Dr. WINTRINGHAM has given us an account of the weather and the correſponding diſeaſes at York for ſixteen years ſucceſſively, in his Commentarium Noſologicum, to which learned work I refer the curious reader for further ſatisfaction upon this ſubject.

Among the general cauſes of our increaſing population and healthineſs we may enumerate the introduction of inoculation; which has been the means of ſaving a number of lives; improvements in the treatment and cure of ſeveral diſorders, the cool regimen in fevers, the admiſſion of freſh air, the general uſe of antiſeptic medicines and diet, have doubtleſs had a ſalutary and extenſive influence upon the health of mankind, and have much obviated the malignity of ſome of our moſt dangerous diſeaſes. To theſe may be added a general improvement and greater attention to nature in the management of infants.

After the general cauſes of healthineſs, ſuch as are particular, or of a more local nature, come under conſideration. In this reſpect the city of York has been much improved within a few years paſt. The ſtreets have been widened in many places, by taking down a number of old houſes built in ſuch a manner as almoſt to meet in the upper ſtories, by which the ſun and air were almoſt excluded in the ſtreets and inferior apartments. They have alſo been new paved, additional drains made, and, by the preſent method of conducting the rain from the houſes, are become much drier and cleaner than formerly. The erection of the locks, about four miles below the city, has been a great advantage to it: for, before this, the river was frequently very low, leaving quantities of ſludge and dirt in the very heart of the city, alſo the filth of the common ſewers which it was unable to waſh away. The lock has effectually prevented this for the future, by the river being kept always high, broad, and ſpacious; and has thus contributed to the ſalubrity as well as beauty of York. In the above improvements, in others that are intended to take place, in the care and expence neceſſary to keep in proper repair the public walks about the city, the magiſtrates have exerted much public ſpirit, and have added to the health as well as conſulted the convenience of its inhabitants.

York,
Sept. 8, 1781.

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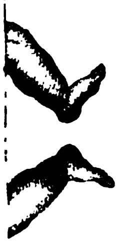
IV. *Account of a monſtrous Birth. In a Letter from* John Torleſe, *Eſq. Chief of* Anjingo, *to the Hon.* William Hornbey, *Governor of* Bombay; *communicated by Dr.* Lind, *F. R. S.*

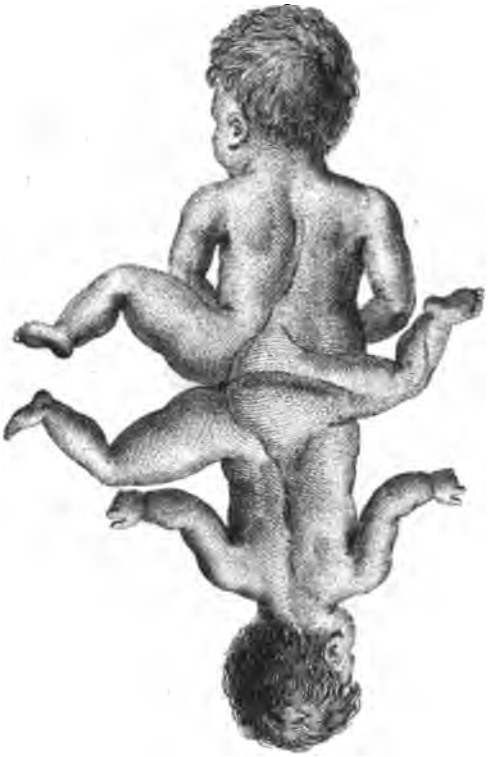
Read January 7, 1782.

HON. SIR,

Anjingo,
April 5, 1780.

AS I knew you are curious with reſpect to the productions of nature, I have taken the liberty to incloſe you a drawing of a child which a Nair woman was delivered of the 18th of March at midnight, and which lived till the 1ſt of April in the morning. In the afternoon I went to ſee it in company with Mr. HUTCHENSON and Dr. CROZIER. You will ſee by the ſketch that it had but one body, at the extremity whereof were two heads, one larger than the other. It had four hands and arms perfect, two legs on one ſide its body, and one on the other, which began on the middle of its back, and appeared by nature intended for two by its ſize and from the appearance of the foot, which looked as if two had been ſqueezed or rather maſhed together. It had but one navel and one anus, but two genitals of the female. It was fed during





Basire s.

its ſhort exiſtence by hand with goat's milk. It is remarkable, that one head would ſleep whilſt the other was awake; or one would cry, and the other not. They both died at the ſame inſtant. Almoſt all this town went to ſee it, the like having never been heard of before. The mother is a ſtout woman; and I ſaw four of her children at her houſe, the youngeſt of which was ſix years old, all healthy and perfect.

I am, &c.

V. *Experiments with* Chineſe Hemp Seed. *In a Letter from* Keane Fitzgerald, *Eſq. to Sir.* Joſeph Banks, *Bart. F. R. S.*

Read January 17, 1782.

SIR,

Poland Street,
Dec. 17, 1782.

EVERY thing extraordinary in art or nature falls, in ſome meaſure, within the views of the Royal Society; but how far the following account of what appeared to me an extraordinary production may be worthy of being communicated to that learned body, is ſubmitted entirely to your conſideration.

A few grains of Chineſe hemp-ſeed had been given to me by the late Mr. ELLIOT, brother to General ELLIOT, who had formerly reſided for ſome time in China. He told me, the hemp in that country was deemed ſuperior to that of any other, both for fineneſs and ſtrength, and wiſhed I would try whether it would come to maturity in this kingdom. He gave me between thirty and forty grains of ſeed for the purpoſe, which I laid by, as I thought, carefully, with intent of ſowing them the ſpring following, which is the uſual time of ſowing hemp in this country; but I had unluckily forgotten where I laid them, and did not find them till the beginning of laſt June, by which time I imagined them to be very unfit for vegetation; but as I concluded they would be ſtill more ſo by keeping them till the ſucceeding April, I had them ſowed the 4th day of that month, and was much ſurpriſed to find that thirty-two of the ſeeds had vegetated ſtrongly, and grown to an amazing ſize, ſeveral of the plants meaſuring in height more than fourteen feet, and ſeven inches nearly in circumference, by the middle of October following, at which time they came into bloom. There were from thirty to forty lateral branches on a plant; theſe were ſet off in pairs, one on each ſide of the ſtem pointing horizontally; the others at about five or ſix inches diſtance from them, pointing in different directions, and ſo on to the top, the bottom branches of ſome meaſuring more than five feet, the others decreaſing gradually in length towards the top, ſo as to form a beautiful cone when in flower, which were unluckily nipped by a few nights froſt that happened to be pretty ſharp towards the end of the month; and the plants began to droop at the beginning of November at which time I had them pulled up by the roots.

As I was but little acquainted either with the cultivation of the ſeed, or preparing the plants afterwards for the production of hemp, and as theſe plants were very different in their ſize from any I had ever ſeen, the beſt method that occurred to me was, that of ſteeping them in water, where I let them remain for a fortnight, and then placed them in an upright poſition againſt a ſouth wall to dry and bleach.

On trying whether the hemp could be eaſily ſeparated from the woody part, I was agreeably ſurpriſed to find, that on peeling a few inches longitudinally from the root, the whole rind, from the bottom to the top, not only of the ſtem but alſo of all the lateral branches, ſtripped off cleanly, without breaking any one of them. The toughneſs of the hemp ſeemed to be extraordinary, and upon drying and beating divides into an infinity of tough fibres. The plants when ſtripped are quite white, and when the lateral branches are cut off, appear like handſome young poles. They are perforated in the middle, but the perforation is not larger than that of a gooſe quill, in a ſtem of more than two inches diameter. The woody part ſeems pretty ſubſtantial, and if they ſhould be found of any duration, might be applied to many uſeful purpoſes; or if not, I ſhould imagine they would produce plenty of good aſhes by burning.

The rough hemp that has been pealed from the thirty-two plants, when thoroughly dried, weighed three pounds and a quarter; but I do not think it had come to full maturity, though I can hardly doubt but the plants would have come to perfection if the ſeed had been ſown in the proper ſeaſon. The ſummer was remarkably dry, notwithſtanding which, although the ſituation they were placed in was very warm, and the ground not rich, I found, on meaſuring the plants at different times, that they had grown nearly eleven inches *per* week.

As the culture of ſo valuable a kind of hemp as this promiſes to produce appears to be of conſequence to a maritime and commercial kingdom, I have applied to the Directors of the Eaſt India Company, to give proper orders to their factors and ſuper-cargoes in China, to procure ſome of the beſt ſeed that can be obtained; and ſend, even a ſmall parcel, by each of their returning ſhips, which they have very obligingly promiſed; and from what has already appeared, there can be no doubt of its continuing in a ſtate fit for vegetation for a much longer time than is uſually required for that voyage.

If the ſeed ſhould arrive in ſafety, I can hardly doubt of obtaining the aſſiſtance of the Society eſtabliſhed for the Encouragement of Arts, Manufactures, and Commerce; and ſhould expect, from their wonted aſſiduity and liberal diſpoſition of proper rewards for the culture and manufacture of ſo valuable a commodity, to ſee it as ſucceſsfully carried to perfection as ſeveral other branches have happily attained by their care and protection; and ſhall think myſelf very happy in being any ways inſtrumental in forwarding ſo good a purpoſe.

As you did me the favour of examining the plants in a growing ſtate, I need not trouble you with any ſpecimens from them; but if they ſhould be deemed worthy of being laid before the Society, I ſhall ſend ſome of the hemp in the ſtate it was peeled, and a piece of the ſtem it was peeled from, as alſo ſpecimens of the leaf and flower, for their examination.

I am, &c.

VI. *An Account of ſome Scoria from Iron Works, which reſemble the vitrified Filaments deſcribed by Sir* Wiliam Hamilton. *In a Letter from* Samuel More, *Eſq. to Sir* Joſeph Banks, *Bart. P. R. S.*

Read January 17, 1781.

SIR,

IN the very accurate account given of the eruption of Mount Veſuvius in the month of Auguſt, 1779, in a letter from Sir WILLIAM HAMILTON, printed in the Philoſophical Tranſactions, vol. LXX. part I. p. 42. et ſeq. among many other equally curious informations, it is ſaid, "Long Filaments of "vitrified matter, like ſpun-glaſs, were mixed with and fell with the aſhes." And in a note annexed it is alſo ſaid, that "during an eruption of the volcano in the Iſle of Bourbon in "1766, ſome miles of country, at the diſtance of ſix leagues "from the volcano, were covered with a flexible capillary yel-"low glaſs, ſome of which were two or three feet long, with "ſmall vitrous globules at a little diſtance one from the other."

There appeared to me on reading theſe paſſages an exact ſimilarity between theſe productions of the two volcanos and ſome ſcoria I had received from a worthy friend, who is maſter of one of the largeſt works in England for ſmelting iron. In a letter accompanying the ſpecimen, he writes, "I have ſent "a ſpecimen of ſome ſlag, or vitrified cinder, which has by "the reverberation of the blaſt from the Tweer *, been drawn "out whilſt fluid into long cobweb-like threads (ſometimes "ten or twelve feet in length) and affixed itſelf to the beams, "&c. of the bellows room."

Whoever has attentively viewed the large furnaces wherein iron ore is ſmelted by coak, will readily allow, that they preſent the moſt ſtriking reſemblance (however diminiſhed) of that moſt tremendous of all appearances, the eruption of a volcano; and that the moſt exact pictures hitherto ſeen of the flowing of the lava from the one is ſhewn by the running of the ſlag from the other: this has induced me to lay before you, for the inſpection of the Royal Society if you judge it worthy their attention, ſome of the ſcoria in its capillary ſtate, and with all due deference to the acknowledged abilities of Sir WILLIAM HAMILTON, to ſubmit to your conſideration, and that of the learned Body over which you ſo deſervedly preſide, whether the fine filaments may not be produced in the eruption of the great furnaces of nature, by means ſimilar to thoſe by which we ſee them formed in the furnaces of art. Sir WILLIAM ſeems to think, "That (what he calls) the natural ſpun glaſs which "fell at Ottaiano, as well as that which fell in the Iſle of "Bourbon in 1766, muſt have been formed, moſt probably, "by the operation of ſuch a ſort of lava as has been juſt de"ſcribed (that is, perfectly vitrified) cracking, and ſeparating "in the air at the time of its emiſſion from the volcanos, and "by that means ſpinning out the pure vitrified matter from "its pores or cells, the wind at the ſame time carrying off thoſe "filaments of glaſs as faſt as they were produced." See p. 81.

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* The Tweer is that opening through which the air is driven by the bellows into the body of the furnace.

That ſome of the fine filaments found after the eruptions of the volcanos were formed in this manner is not unlikely: but as we ſee about the iron furnaces the vitrified ſcoria drawn into fine threads, of very conſiderable length, by the ſimple action of the wind from the bellows, is it not very probable, that the far greater part at leaſt of thoſe filaments ſcattered over the land, and which were found two or three feet long, were drawn out before the ejection of the lava from the crater by the force of thoſe violent torrents of wind which muſt be required to ſupport and actuate ſo intenſe a fire as at thoſe times fills the body of the mountain ?.

In all matters of this kind there is great ſcope for conjecture, and much muſt be allowed to it; and I have preſumed to ſubmit this opinion to you, not with an intention to diſpute the probability of what has been already advanced on this head, but to point out from what occurs immediately under the eye of every workman about our iron furnaces, ſome eaſy and ſimple mode of accounting for ſo ſingular a phenomenon, and as an introduction to my preſenting to the Royal Society a ſpecimen of ſo curious a production.

The extreme fineneſs to which theſe filaments are reduced, and their brittleneſs, render it almoſt impoſſible to convey them to any diſtance, preſerving at the ſame time any conſiderable length of the fibres; theſe which I have now the honour to lay before you reſemble cotton in appearance, but if examined with a microſcope will be found in all reſpects ſimilar to thoſe deſcribed by Sir WILLIAM HAMILTON.

I am, &c.

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VII. *An Extract of the Regiſter of the Pariſh of* Holy Croſs, Salop, *being a Third Decade of Years from* Michaelmas 1770 *to* Michaelmas 1780, *carefully digeſted in the following table. By the Rev. Mr.* William Gorſuch, *Vicar; communicated by Dr.* Price, *F. R. S.*

Read January 17, 1781.

		1771	1772	1773	1774	1775	1776	1777	1778	1779	1780		
Baptized	Males	23	20	19	20	18	31	16	18	20	18	203	385
	Females	16	18	16	12	23	14	17	27	22	17	182	
Buried	Males	16	19	12	11	13	28	13	12	23	13	160	311
	Females	13	20	17	10	6	21	14	15	13	22	151	
											Increaſe	74	

	1771		1772		1773		1774		1775		1776		1777		1778		1779		1780		Total	
	M	F	M	F	M	F	M	F	M	F	M	F	M	F	M	F	M	F	M	F	M	F
Under a month	0	1	—	—	1	1	2	1	1	0	2	2	—	—	1	1	4	3	0	2	11	11
From mo. to 1 yr.	2	3	2	2	0	3	0	3	0	1	2	2	2	1	2	2	4	1	1	5	15	23
1 to 2	1	0	1	3	2	3	—	—	0	1	4	4	—	—	—	—	2	0	1	0	11	11
2 to 5	1	0	4	2	1	2	—	—	1	0	5	7	2	3	2	1	1	0	—	—	17	15
5 to 10	—	—	2	2	0	1	—	—	0	1	3	1	0	2	2	0	—	—	—	—	7	7
10 to 15	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	0	2	1	0	1	2
15 to 20	—	—	1	2	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	2
20 to 25	1	0	—	—	—	—	—	—	0	1	1	0	—	—	1	1	1	0	—	—	4	2
25 to 30	—	—	1	0	—	—	—	—	3	0	1	1	1	1	0	1	0	1	1	1	8	4
30 to 35	—	—	—	—	1	2	1	0	—	—	—	—	0	1	0	1	1	0	—	—	3	4
35 to 40	2	0	—	—	—	—	1	2	2	0	1	1	—	—	1	0	—	—	1	0	8	3
40 to 45	2	2	0	1	1	0	1	0	1	0	—	—	—	—	—	—	0	1	2	0	7	4
45 to 50	2	0	1	1	0	2	—	—	—	—	—	—	0	1	0	1	—	—	—	—	3	5
50 to 55	1	0	1	1	1	1	1	2	1	0	1	1	2	0	0	1	3	1	0	1	11	8
55 to 60	2	1	—	—	—	—	—	—	1	0	—	—	1	0	1	0	—	—	0	3	5	4
60 to 65	0	1	0	1	0	1	2	0	1	0	3	0	1	2	0	2	3	0	3	2	13	9
65 to 70	—	—	1	0	—	—	—	—	—	—	—	—	—	—	2	2	—	—	0	1	3	3
70 to 75	0	2	1	3	4	0	1	1	1	2	2	0	—	—	0	2	0	2	2	3	11	15
75 to 80	2	1	1	1	—	—	—	—	1	0	2	0	0	1	—	—	2	1	1	3	9	7
80 to 85	0	2	2	1	1	0	2	0	—	—	1	2	4	1	—	—	2	1	—	—	12	7
85 to 90	—	—	1	0	—	—	—	—	—	—	—	—	0	1	—	—	—	—	—	—	1	1
90 to 95	—	—	—	—	0	1	0	1	0	1	—	—	—	—	—	—	—	—	0	1	0	4

An actual ſurvey was made in 1775, when the number of the inhabitants was found to be total 1057: of which under ten 287, and above ſeventy 57, *viz.* from 70 to 75, males 12 females 10 = 22. From 75 to 80, males 8 females 11 = 19. From 80 to 85, males 8 females 6 = 14. From 85 to 90, males 1 females 1 = 2.

An actual ſurvey was made in the year 1780, when the number of inhabitants were 1113.

There remains alive in 1780.

Under ten years of age,	males	155	293
	females	138	
From 70 to 75	males	6	17
	females	11	
From 75 to 80	males	5	13
	females	8	
From 80 to 85	males	2	6
	females	4	
From 85 to 89	males	2	3
	females	1	

Diſtempers

Diſtempers and Caſualties from 1770 to 1780.

Accidents	6	Meazles	7
Apoplexy	5	Palſy	9
Aſtma	5	Pleureſy	1
Cancer	2	Rheumatiſm	1
Chin-cough	5	Small-pox	43
Conſumption	62	Sore-throat	8
Child-bed	3	Stone	2
Convulſions	23	Suddenly	2
Dropſy	20	Teeth	2
Drowned	3	Untimely	4
Fever	15	Worms	3
Jaundice	2		
Lues venerea	1		
Mortification local	5		
Mortification inteſtine	10		

The remainder died of a natural decay, without any oſtenſible Diſtemper.

The number of inhabitants actually ſurveyed every five years for thirty years.

In 1755	-	1049
1760	-	1048
1765	-	1096
1770	-	1046
1775	-	1057
1780	-	1113

The increaſe of 48 perſons in the year 1765 was owing to the ingreſs of four numerous families into large houſes, which were almoſt uninhabited for many years before.

The decreaſe of 50 perſons in the year 1770 was occaſioned by the demoliſhing of nine houſes, in order to open a way to the new ſtone bridge built over the river Severn.

On Good Friday, 1774, there happened a dreadful fire which originated from a chimney, and extended on both ſides of the ſtreet to the diſtance of half a mile: the wind blowing with great violence, the flames in a few hours conſumed 48 houſes, being generally thatched buildings. In this conflagration 179 of the inhabitants loſt their dwellings, but immediately provided themſelves with lodgings within the pariſh, and of the number of ſufferers only 24 perſons went out of the pariſh, and returned no more. The ground vacated by the houſes burnt is now, in 1780, built upon, and moſtly covered with little tenements fitted for poor inhabitants to live in, and made ſo commodious as to receive a greater number of inhabitants than they did before the fire in 1774; ſo that the families, whoſe number in 1770 were 240, are in the year 1780 increaſed to 246 nearly, and perhaps will be increaſing when more buildings ſhall be erected.

Houſes pay window lights 65. The new houſe tax paid by 36 houſes. The firſt decade was publiſhed in the Phil. Tranſ. vol LII. part I. art. 25. The ſecond decade was publiſhed in the Phil. Tranſ. vol. LXI. art. 6. p. 57. See alſo Dr. PRICE's Obſervations on Reverſionary Payments, ed. 1771. p. 192. and note *a*. alſo p. 259. and 263.

The taking account of the marriages in this pariſh cannot be of any uſe in political arithmetic, becauſe it is the cuſtom of the fixed inhabitants to go out of the pariſh, and be married in diſtant churches; and the weddings performed in this church are generally between ſtrangers who occaſionally reſide here ſo long as to make a place of abode according to the act of parliament made in 1754. Dr. HEBERDEN hath made a very proper uſe of the number and proportion of marriages in the iſland of Madeira; but then it was an iſland, and all are confined to their own conſtant reſidence. If the whole iſland of Great Britain was univerſally to be included in the account of the number of marriages, it would be very uſeful and compleat.

Many young people have gone out of this pariſh to ſupply the navy and army, but probably the ſame number would have emigrated, to be apprenticed and follow different occupations.

The public regiſter is too general for the purpoſes of political arithmetic. The extracts here made are drawn from private papers, kept with great care and exactneſs, ſo that the births and burials of the fixed inhabitants are not rendered incorrect by the accidental ingreſs of foreigners, or temporary egreſs of the real inhabitants.

VIII. *An Experiment propoſed for determining, by the Aberration of the fixed Stars, whether the Rays of Light, in pervading different Media, change their Velocity according to the Law which reſults from Sir* Iſaac Newton's *Ideas concerning the Cauſe of Refraction; and for aſcertaining their Velocity in every Medium whoſe refractive Denſity is known. By* Patrick Wilſon, *A. M. Aſſiſtant to* Alexander Wilſon, *M. D. Profeſſor of Practical Aſtronomy in the Univerſity of* Glaſgow; *communicated by the Rev.* Nevil Maſkelyne, *D. D. F. R. S. Aſtronomer Royal.*

Read January 24, 1782.

UPON the ſuppoſition that the refraction of light is cauſed by a certain action of groſs and ſenſible bodies upon it, Sir ISAAC NEWTON has demonſtrated, that the ſines of incidence and refraction, when the rays paſs out of one medium into another of different denſity, muſt always be in a conſtant ratio. This conſtancy of the ratio of the ſines is agreeable to an univerſal experience, and has been called the law of refraction. Upon the ſame grounds he has alſo demonſtrated, that the velocity of the rays muſt be greater in the more refracting medium in the inverſe ratio of the ſines. Of this property of refraction, however, we have hitherto had no evidence in the way of experiment. The ideas entertained by Sir ISAAC NEWTON, from which this property has been deduced, though they confeſs their great author, by a moſt beautiful ſimplicity, and by a very ſtriking agreement with fact, have yet been deemed by ſome perſons as not perfectly authentic. His contemporary LEIBNITZ and others have attempted demonſtrations of the law of refraction from principles very different, and which do not lead to the opinion of the acceleration of light in the more refracting medium. At preſent it is propoſed to point out a method of determining experimentally the law of the variation of the velocity of light, according to the change of the medium. If obſervations ſhall ſhew this law to be agreeable to Sir ISAAC NEWTON's concluſions, we ſhall then have a very ſtrong additional evidence in favour of his principles. If, contrary to the moſt probable iſſue of the experiment, ſome unſuſpected law ſhould be diſcovered, we muſt, according to the rules of induction laid down by that great maſter in philoſophy, ſo far reſtrict our general concluſions, and accommodate our ideas to the real condition of things.

The method of experiment at preſent alluded to is, that of obſerving the aberration of the fixed ſtars with a teleſcope filled with a denſe fluid, ſuch as water, or any other equally limpid and of greater refraction, fitted to bring the rays to a focus by the ſurface of the medium oppoſed to the object having a proper degree of convexity. It is enough at this time to ſuggeſt a general notion of the inſtrument, and we now proceed to explain in what manner it can aſſiſt us in the preſent inquiry.

Since aberration, taken in its enlarged ſenſe, depends on the relative velocities of light and of the teleſcope, if the rays were really to move much faſter or much ſlower in an unuſual teleſcope of this kind, it ſeems to follow, that the quantity of aberration given in theſe circumſtances, compared with Dr. BRADLEY's angle, would certainly indicate the new rate of velocity. Such an inference would certainly be juſt, and it is upon theſe grounds that we propoſe to inquire into the velocity of the rays, as they move forward in denſe media ſo applied to teleſcopes. Granting, however, for the ſake of argument, that light moves down through ſuch an unuſual teleſcope with an increaſed velocity ſuited to the refractive denſity of the medium, it will by no means happen, that the aberration will be changed on that account. This propoſition, which at firſt view may appear paradoxical, and even contradictory to what has been affirmed above, is however not the leſs certain, and may ſerve to ſhew, what caution is ſometimes requiſite in applying general principles to particular caſes: for it ſhall be proved, that the aberration in ſuch a teleſcope will preciſely agree with that of Dr. BRADLEY's only in the caſe of the rays moving ſwifter in the watery medium than in air, in the ratio aſſigned by Sir ISAAC NEWTON, and that this ſameneſs of aberration will itſelf be a proof of light being ſo accelerated within the teleſcope.

In the illuſtrations which follow, the reader is ſuppoſed not to be wholly unaccuſtomed to the diſtinctions betwixt abſolute and relative motion, as this will prevent repetitions and all unneceſſary prolixity.

Let ABC (fig. 1.) be the ſpherical refracting ſurface of ſuch a teleſcope as has been deſcribed, and let the teleſcope be ſuppoſed to be at reſt, or the velocity of light to be infinite with reſpect to that of the earth, and let GBMF be a line drawn from a ſtar at G, in the pole of the ecliptic, through the center M of the refracting ſurface; the image of the ſtar will be formed ſomewhere, as at F, in the line BF; and here the interſection of the croſs wires made uſe of in obſerving muſt be placed. It is evident, that the ſtar will be ſeen in its true direction FG; and we muſt conclude that to be its true direction, becauſe we know that the ray GBF paſſes into the medium without being refracted by it, and BMF would be conſidered as the axis of the teleſcope.

Now let the ſpherical refracting ſurface with its wires, or the unuſual teleſcope be carried laterally with the motion of the earth towards Q. Conceive GBF to be a line not partaking of this lateral motion, which at any particular moment paſſes thro' M, the center of convexity. Along this line ſuppoſe one of many rays to paſs from a ſtar ſituated in the pole of the ecliptic. Then will all the contemporary light of this pencil of parallel rays be made to converge ſo as to meet in a focus ſomewhere in the unrefracted ray BF. Let F therefore be the point in abſolute ſpace where the image of the ſtar is ſo formed. Let the parallel motion of the teleſcope, whoſe refracting ſpherical ſurface is ABC, be in the direction of HF, and take FD to FB as the lateral velocity of the teleſcope to the velocity of light in air, and join BD: then it is manifeſt, that BD will be the poſition of a teleſcope ſuch as Dr. BRADLEY's, when the image of the ſtar is formed in the axis BD, and that IBG, or its equal FBD, will be the angle of greateſt aberration.

Moreover, the velocity of the rays as they proceed to the focus F, after refraction at the ſurface ABC, being ſuppoſed the ſame as in air, it is evident, that the line DML drawn through D, and through the center of convexity M, muſt give the poſition of the axis of this kind of teleſcope, when the image of the ſtar is formed there: for, by hypotheſis, the image is formed in F in abſolute ſpace, and ſince BF is ſuppoſed to be to FD, as the velocity of light within the medium to the lateral velocity of the teleſcope, the point D of the axis DL will arrive at F, when the rays arrive there to form the image. And the obſerver not knowing, or at preſent not taking account of, the lateral motion of the teleſcope, will ſuppoſe, that the line LMD joining the image of the ſtar and the center of convexity M is the true direction of the ſtar; juſt as before he concluded, that FMBG would be the direction of the ſtar when the lateral motion of the teleſcope was ſuppoſed to be nothing. Hence it is evident, that the interſection of the croſs wires, made uſe of in obſerving, muſt now be placed at D; or elſe, if thoſe be ſtill uſed that were before ſuppoſed to be at F, the refracting ſurface ABC with the line or axis BF muſt revolve about the center M till the vertex B comes to L and the croſs wires F to D.

In like manner, if the velocity of the rays were increaſed after refraction at the ſpherical ſurface in any ratio, as that of DF to EF, the refraction continuing the ſame, then EMO drawn through the center of convexity would now give the poſition of the axis of the teleſcope neceſſary for receiving the image formed at F. For the ſpace deſcribed by the rays in paſſing downwards to the focus, in this caſe and the former being equal, the times of their converging at F will be reciprocally as the velocities, or as EF to DF. But, on account of the equable lateral motion of the teleſcope, DF and EF will be as the times of the points D and E arriving at F: therefore, in the laſt caſe, the interſection of the croſs wires ſuppoſed at E will meet the image at F, and accordingly the ſtar will be ſeen in the axis.

Fig. 2. From what has been ſaid it will appear, that if DF be taken to EF, as the ſine of incidence to the ſine of refraction peculiar to the medium which fills the teleſcope; then, from the property of the focus, we ſhall have this proportion, *viz.* BF : FM :: DF : EF. Hence the line EMO paſſing

through M muſt be parallel to DB; but DB, as before, denotes the poſition of Dr. BRADLEY's teleſcope, when the aberration of the ſtar is at its maximum, and EMO parallel to it, denotes the poſition of the water teleſcope, at the ſame time, upon the ſuppoſition that the velocity of the rays without and within be as EF to DF, or inverſely, as the ſines of incidence and refraction peculiar to water. Here then we diſcover what muſt be the law of variation as to the velocity of the rays, provided that the aberration given by ſuch a teleſcope ſhall come out the ſame with that found by Dr. BRADLEY. It is the very ſame which follows from the Newtonian principles: for from the manner of obſerving, the angle of aberration is always determined by the poſition of the teleſcope neceſſary for having the image formed ſomewhere in the axis.

But ſuppoſing that in the courſe of obſerving with ſuch a teleſcope, the aberration ſhould come out different from what has already been aſcertained by Dr. BRADLEY, it may next be enquired, how from the difference given the velocity of light within the teleſcope is to be deduced.

Fig. 3. Imagine then ſuch a teleſcope actually to give FMD as the greateſt angle of aberration, and let this be ſuppoſed greater than that of Dr. BRADLEY's, which, for example, let be FME. From what has been already ſaid, the velocity of light correſponding to this laſt mentioned angle, is deducible from the known refraction of the medium which fills the teleſcope; and, by conſtruction, the velocity correſponding to FMD, the angle given, muſt be to the former inverſely as the tangents of theſe angles. From this conſideration we have the following analogy for finding the velocity correſponding to whatever difference there may be obſerved between the two aberrations at preſent alluded to. The rule in all caſes muſt be;

" as the tangent of the obſerved angle is to the tangent of the " Bradleyan angle, ſo is the velocity of light deducible from " the hypotheſis of the obſerved angle being the ſame with " that of Dr. BRADLEY to the velocity ſought." It has already been ſhewn, how the former of theſe velocities can be univerſally aſcertained, from the known refraction of the medium which is taken to fill the teleſcope, and therefore the laſt term of the above proportion, which is the velocity ſought, is thereby given.

Fig. 2. In a teleſcope of this kind it will not have eſcaped notice, that the ray BF, which, on account of its paſſing to the focus unrefracted, may be called the axis of the pencil, can never be found in the axis of the teleſcope EO, except at the focus F, where D and F meet. That ray, however, OP, parallel to BG, which falls obliquely on the axis of the teleſcope EO, will continue to paſs along it after refraction, and for that reaſon it may be called the relative axis of the pencil.

This will appear, by conſidering that the particle of light, which at any moment is refracted at the vertex O of the ſpherical ſurface, is found by hypotheſis in the axis a ſecond time, when it meets the cotemporary light at the focus. But ſince both the motion of the axis and of the particle is uniform and rectilinear, the former cannot be found in the latter at two different times, without being found in it continually during the whole interval. In like manner, a part of every other ray from the ſtar, which ſucceſſively falls upon the vertex, muſt move relatively along the axis after refraction: and thus a conſtant ſucceſſion of theſe particles conſtitute a viſual refracted ray, whoſe relative path muſt always be in the axis OE.

All that has been ſhewn concerning the teleſcope already conſidered, will receive ſtill further illuſtration, by tracing the motion of this particular refracted ray till it arrives at the focus. This way of viewing the ſubject will alſo render the reaſoning more general, and make it apply to teleſcopes when the denſe fluid within is ſuppoſed to be confined by object-glaſſes of any figure. But in order to this, it will be convenient to premiſe, and briefly to demonſtrate, what ſhall afterwards be referred to by the name of

PROP. A.

Fig. 4. If any very ſmall body or particle of light as it moves uniformly in the abſolute path SB, has paſſed relatively along a part of the line CD, which advances equably and parallel to itſelf in the direction DK; and if at any inſtant the abſolute path of the particle be changed into any other, as BR; I ſay, it will ſtill paſs relatively along the moving line, provided its velocity now be to its former velocity as the ſine of the angle DBF to the ſine of the angle DBR; theſe being the angles which the moving line BD makes with BF and BR the abſolute path or direction of the particle in the two caſes.

The conſtruction of this figure is ſo ſimple, that it is unneceſſary formally to point it out. Since, by hypotheſis, the velocity of the particle along BR is to its former along BF as the ſine FZ to the ſine RT; or, on account of ſimilar triangles, as DF to IR, and, on account of parallels, as DF to DW, it follows, that the time of its deſcribing BR now, is to the time of formerly deſcribing its equal BF, as DW to DF. But the line BD advancing with a uniform motion, the time of its arriving at W is to the time of its arriving at F, alſo as DW to DF. Therefore, when the particle arrives at R, the point D of the moving line will have arrived at W, and WRP will be its poſition. Hence the particle at that moment muſt be found in the interſection R of this line, with its abſolute path BR. In the ſame manner it may be ſhewn, that at any other time the particle will be found in the interſection: it, therefore, from the time of its direction being changed at B, muſt paſs relatively along the moving line as before. By a ſmall alteration in the conſtruction it may be ſhewn, that if the abſolute path had been ſo changed at B as to have augmented the angle FBD, ſtill the particle would have moved relatively along DB, provided its velocity after had been to its velocity before as the ſine of FBD the firſt angle to the ſine of the increaſed angle.

To apply, therefore, this propoſition to the preſent inveſtigation, let DB be conceived as the axis of a teleſcope perpendicular to the ſpherical ſurface of a refracting medium which accompanies it in its lateral motion, SB the abſolute path of a particle of light which had paſſed relatively along DB produced, till its arrival at B, and BR its abſolute path within the medium of the teleſcope. Then it is evident, that FBD, or its equal CBS, will be univerſally the angle of incidence, and RBD the angle of refraction. Hence, by prop. A. that ray of the parallel pencil which is refracted at O, the vertex of the ſpherical ſurface in fig. 2. muſt ſtill paſs relatively along the axis, provided the velocity within the teleſcope be to its former in air, as the ſine of incidence to the ſine of refraction. But the image of the ſtar being produced by the meeting of all the contemporary light, will conſequently be found in the axis, which, by hypotheſis, deviates from the true place of the ſtar by the ſame quantity as Dr. BRADLEY's angle; ſo that in this way of conſidering the matter, the ſame thing reſults which was formerly ſhewn in regard to a teleſcope ſo conſtructed.

By prop. A. it is alſo manifeſt, that whatever number of refractions that ray which falls upon the extremity of the axis ſuffers in pervading object-glaſſes of any figure, or even denſe media beyond the object-glaſs if bounded by tranſparent planes to which the axis produced is perpendicular, yet if the velocities and refractions ſo correſpond, ſtill the ray in queſtion will paſs relatively along the axis till it meet the reſt at the focus: for here the refracted ray in the firſt medium becomes the incident ray in relation to its path in the ſecond, and this in its turn becomes an incident ray in relation to its path in the third medium, &c. and therefore by the prop. A. can never deviate from the moving axis whatever be the refractive denſity of the media, or however theſe are diſpoſed in the order of ſucceſſion. And ſince, by Sir ISAAC NEWTON's theorem, the ratio of the ſine of incidence to the ſine of refraction in the paſſage of a ray out of one medium into another, is compounded of the ratio which the former has to the latter in the paſſage of that ray out of the firſt medium into any third, and of the ratio of the former to the latter in the paſſage of the ſame ray out of the third medium into the ſecond, &c. it follows, that if the velocities be related to the degree of refraction as before mentioned, the ray in the laſt denſe medium will, notwithſtanding any number of previous refractions by glaſſes, &c. have the ſame final velocity that would have been acquired on its paſſing immediately out of air into that medium. This being the caſe, it appears, that though the intervention of an object-glaſs may ſhorten the focal diſtance of ſuch a teleſcope, yet it will not diſplace the image nor alter the rule of inferring the final velocity of the rays in the denſe medium from the aberration given; at leaſt when this is ſuppoſed to be the ſame with Dr. BRADLEY's.

Fig. 3. But further, if the aberration of ſuch a teleſcope ſhould differ from the Bradleyan one, and give, for example, the angle OMB, ſtill the ray PO, which falls on O the vertex, muſt be conſidered as an incident ray, which, after refraction,

paſſes along the axis. By prop. A. therefore, the velocity of the ray, whatever this may be after refraction, muſt be to that velocity by which it would have moved relatively in the axis, ſo inclined to its path, previous to the refraction, inverſely as the ſines of incidence and refraction. Now this being duly conſidered, it will be found that the velocity within the medium, correſponding to this ſuppoſed aberration, or the abſolute velocity within the medium, muſt be to the velocity within the medium correſponding to the Bradleyan aberration, inverſely as the tangents of theſe two angles: for let V and v expreſs the velocities before and after refraction correſponding to the Bradleyan angle, and X and x the velocities before and after correſponding to the ſuppoſed uncommon angle, x being the actual velocity after refraction; then, becauſe by prop. A. the antecedent is to the conſequent, in both caſes, in the ſame ratio, *viz.* as the ſine of refraction to the ſine of incidence, it will be $V : v :: X : x$, and therefore $V : X :: v : x$. But from the nature of the aberration V muſt be to X (this ſuppoſititious velocity before incidence) inverſely as the tangents of the angles of the two aberrations. This therefore muſt be the ratio of v to x. But v is given as before ſhewn; therefore x the velocity within the medium correſponding to the ſuppoſed obſerved aberration is alſo given, and by the ſame rule as was found formerly in the caſe of the firſt teleſcope.

What has been at preſent advanced is unconnected with any hypothetical notions concerning the rays or the cauſe of refraction. Light has been conſidered only as ſomething which moves uniformly from one place to another, and which is always refracted according to a known law. The firſt of theſe properties has been put beyond all doubt by the obſervations of Dr. BRADLEY and Mr. MOLYNEUX; and it is has been long known that the laſt is quite agreeable to experience.

It has indeed always been taken for granted, that the velocity of the ray which paſſes through the center of convexity, repreſents the common velocity of all the contemporary light of the converging pencil. This may perhaps be reckoned a circumſtance of which we have no proof. But it muſt be conſidered, that if the rays of light, after being variouſly bent towards the focus, were no longer to move with the ſame common velocity, the image formed at the focus of Dr. BRADLEY's teleſcope, would be elongated in the direction of the aberration. Thoſe who have attended to this ſubject will be at no loſs in diſcerning the reaſon of this. The extent of that lengthened image would depend upon the difference of velocity which would obtain among the converging rays, and would probably increaſe according to the largeneſs of the aperture of the object-glaſs. But ſuch a phenomenon being contrary to experience, it follows, that the unequal bending of the rays does not give them unequal velocities, whilſt moving in the ſame medium. This is another property with regard to the motion of light which may be conſidered as proved experimentally by Dr. BRADLEY's obſervations, and which doubtleſs would have occurred to him if he had had occaſion to trace the refraction of a pencil of parallel rays at the object-glaſs of his teleſcope.

To conclude: in bringing this queſtion concerning the velocity of light to the iſſue of an experiment, that fluid would doubtleſs be moſt proper for the teleſcope which abſorbs the feweſt rays, and poſſeſſes the greateſt refractive denſity, and which at the ſame time is not liable to generate air-bubbles. To compenſate for the unavoidable loſs of light, which by Mr. CANTON and Dr. PRIESTLEY's experiments is found to be conſiderable in ſuch caſes, it perhaps may be neceſſary to uſe an achromatic object-glaſs for the ſake of a large aperture, and of ſuch a figure as to ſhorten the focal diſtance as much as the obſervations of ſuch a ſmall angle can admit of. Some contrivance too will be requiſite to keep the whole ſpace between the object-glaſs and the eye-glaſs always full, notwithſtanding the expanſions and contractions of the confined fluid by heat and cold, or its waſte by evaporation.

It might prove a very conſiderable abridgement of the neceſſary apparatus, if this kind of teleſcope could be connected with the common teleſcope of a mural quadrant, or zenith ſector, and their axes made perfectly parallel by previous obſervations of a proper terreſtrial object. But as there would be ſome room for apprehending that the exact adjuſtment of the axes might be affected in raiſing the teleſcopes afterwards for celeſtial obſervations, this might be examined into by directing them to ſome ſtar ſituated in, or very near, the ecliptic, and taking its meridian altitudes at a time of the year when it is in quadrature with the ſun, in which caſe it would have no aberration. But either in this way, or with two ſeparate inſtruments, the experiment might be made in a few nights, by taking the zenith diſtance of a proper ſtar, the plane of the inſtruments being alternately turned different ways in obſerving, to get the true zenith diſtance independent of the error of the line of collimation; or the meridian altitude of the pole ſtar may be obſerved in December above and below the pole, which will give the apparent diſtance of the ſtar from the pole at that time as affected by aberration. The error of the line of collimation would not affect the reſult in this way, being the ſame in the obſervation both above and below the pole.

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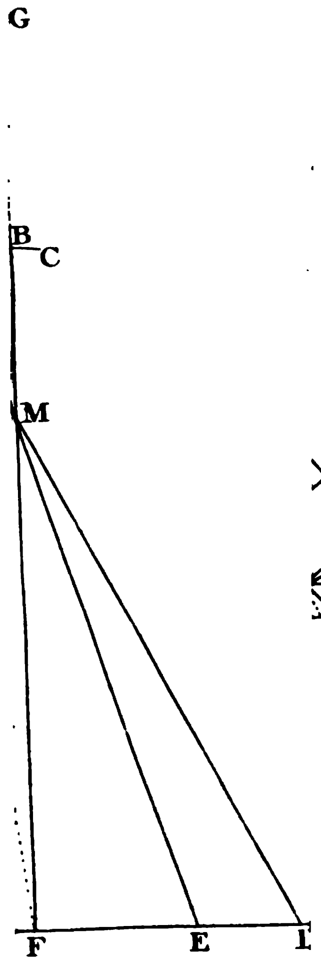
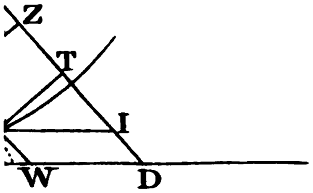
Philos. Trans. Vol. LXXII. Tab. II. p. 70.





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IX. *Quantity of Rain which fell at* Barrowby *near* Leeds *. *By* George Lloyd, *Eſq. F. R. S.*

Read January 31, 1782.

	1778	1779	1780	1781	Averages.
	Inches.	Inches.	Inches.	Inches.	Inches.
January	2.1	0.6	0.9	1.2	1.2
February	1.3	0.0	1.4	2.7	1.35
March	2.1	0.4	0.5	0.1	0.775
April	0.3	2.0	4.3	1.5	2.025
May	1.8	3.4	1.4	1.1	1.925
June	2.1	2.15	0.8	3.2	2.0625
July	3.9	5.6	1.3	1.6	3.1
Auguſt	0.33	2.25	2.45	3.4	2.1075
September	2.87	3.55	4.55	5.6	4.1425
October	5.3	2.8	3.15	0.3	2.8875
November	2.6	2.1	1.95	2.8	2.3625
December	3.3	4.2	0.2	2.1	2.45
Totals	28.0	29.05	22.9	25.6	26.3875

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* See Phil. Tranſ. vol. LXVII. part I. p. 512.

X. *Account of an improved Thermometer. By Mr.* James Six; *comunicated by the Rev. Mr.* Wollaſton, *F. R. S.*

Read February 28, 1782.

ATTEMPTING ſome time ago to aſcertain the greateſt degree of heat and cold that happened in the atmoſphere each day and night, or during the courſe of twenty-four hours, I experienced the inconvenience which attends thermometers commonly made uſe of for that purpoſe; the neceſſity I mean of the obſerver's eye being on the inſtrument the very inſtant the mercury ſtands at the higheſt or loweſt degree: for, ſince the time when that may happen is utterly uncertain, if it be not immediately noticed, it can never after be known. The ſultry heat of the ſummer's days, and freezing cold of the winter's nights, which is commonly moſt ſevere at a late unſeaſonable hour, render it very unpleaſant to be abroad in the open air, although it is abſolutely neceſſary for the thermometer to be placed in ſuch a ſituation. Ingenious men of our own country, as well as foreigners, have, it ſeems, long ago endeavoured to remedy this inconvenience; and ſeveral thermometers of different conſtructions have been invented for that purpoſe. VAN SWINDEN deſcribes one which he ſays was the firſt of the kind, made on a plan communicated by Mr. BERNOULLI to Mr. LEIBNITZ. Mr. KRAFT, he alſo tells us, made one nearly like it[*]. A deſcription of thoſe by Lord

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[*] Diſſ. ſur la Comparaiſon du Therm. par VAN SWINDEN, p. 253—255.

CHARLES CAVENDISH and Mr. FITZGERALD may be ſeen in the Philoſophical Tranſactions [*]. Though much ingenuity appears in the invention of thoſe curious inſtruments, I could not forbear thinking, that a thermometer might be conſtructed more conveniently to anſwer the purpoſe, and ſhew accurately the greateſt degree of heat and cold which happened in the obſerver's abſence. I therefore attempted to make one: with what ſucceſs I ſubmit to your better judgement, and proceed to give a deſcription of the inſtrument. Fig. 1. *ab* is a tube of thin glaſs, about ſixteen inches long, and five ſixteenths of an inch in diameter; *cdefgh* a ſmaller tube with the inner diameter, about one fortieth, joined to the larger at the upper end *b*, and bent down, firſt on the left ſide, and then, after deſcending two inches below *ab*, upwards again on the right, in the ſeveral directions *cde*, *fgh*, parallel to, and one inch diſtant from it. On the end of the ſame tube at *h*, the inner diameter is enlarged to half an inch from *h* to *i*, which is two inches in length. This glaſs is filled with highly rectified ſpirits of wine to within half an inch of the end *i*, excepting that part of the ſmall tube from *d* to *g*, which is filled with mercury. From a view of the inſtrument in this ſtate, it will readily be conceived, that when the ſpirit in the large tube, which is the bulb of the thermometer, is expanded by heat, the mercury in the ſmall tube on the left ſide will be preſſed down, and conſequently cauſe that on the right ſide to riſe; on the contrary, when the ſpirit is condenſed by cold, the reverſe will happen, the mercury on the left ſide will riſe as that on the right ſide deſcends. The ſcale, therefore, which is FAHRENHEIT's, beginning with *o* at the top of the left ſide, has the degrees numbered downwards, while that at the right ſide, beginning with *o* at the bottom, aſcends. The diviſions are aſcertained by placing this thermometer with a good ſtandard mercurial one in water gradually heating or cooling, and marking the diviſions of the new ſcale at every 5°*. Thus far our thermometer reſembles in ſome reſpects thoſe of Mr. BERNOULLI and Lord CHARLES CAVENDISH; but the method of ſhewing how high the mercury had riſen in the obſerver's abſence, the eſſential property of an inſtrument of this kind, is wholly different from theirs, and effected in the following manner. Within the ſmall tube of the thermometer, above the ſurface of the mercury on either ſide, immerſed in the ſpirit of wine, is placed a ſmall index, ſo fitted as to paſs up and down as occaſion may require: that ſurface of the mercury which riſes carries up the index with it, which index does not return with the mercury when it deſcends; but, by remaining fixed, ſhews diſtinctly, and very accurately, how high the mercury had riſen, and conſequently what degree of heat or cold had happened. Fig. 2. repreſents one of theſe indexes drawn larger than the real ones, to render it more diſtinct. *a* is a ſmall glaſs tube, three quarters of an inch long, hermetically ſealed at each end, incloſing a piece of ſteel wire, nearly of the ſame length; at each end *c d* is fixed a ſhort piece of a tube of black glaſs, of ſuch a diameter as to paſs freely up and down within the ſmall tube of the thermometer. The lower end, floating on the ſurface of the mercury, is carried up with it when it riſes, while the piece at the upper end, being of the ſame diameter, keeps the body of the index parallel to the ſides of the thermometrical tube. From the upper end of the body of the index at *c* is drawn a ſpring of glaſs to the fineneſs of a hair, about five ſevenths of an inch in length, which, being ſet a little oblique, preſſes lightly againſt the inner ſurface of the tube, and prevents the index from following the mercury when it deſcends, or being moved by the ſpirit paſſing up or down, or by any ſudden motion given to the inſtrument by the hand or otherwiſe; but at the ſame time the preſſure is ſo adjuſted as to permit this index to be readily carried up by the ſurface of the riſing mercury, and downwards whenever the inſtrument is be rectified for obſervation. To prevent the ſpirit from evaporating, the tube at the end *i* is cloſely ſealed *. Fig. 3. repreſents the thermometer on its frame; the plates on which the ſcale is graved on either ſide are made to ſlide out, and the frame is open to the back, behind the large tube, which does not touch it, except at each end. The cap *a*, and the baſe *b*, are made to fix on with ſcrews, and only cover the turning of the ſmall tube. By a ſcrew at the bottom of the frame, it may be made faſt to the wall againſt which it is to hang without doors, to prevent its being ſhaken by violent winds. Towards evening I uſually viſit my thermometer, and ſee at one view, by the index on the left ſide, the cold of the preceding night; and by that on the right, the heat of the day. Theſe I minute down, and then apply a ſmall magnet to that part of the tube againſt which the indexes reſt, and move each of them down to the ſurface of the mercury: thus, without heating, cooling, ſeparating, or at all diſturbing the mercury, or moving the inſtrument, may this thermometer, without a touch, be immediately rectified for another obſervation. When I wiſh to put the thermometer out of my hand, without hanging it up, I have a ſtand to place it on; for if the mercury preſſes againſt the index, while the inſtrument lies in an horizontal poſition, it is in danger of paſſing by it, which is avoided by keeping the thermometer in a poſition nearly vertical. To prevent the mercury ſhifting its place in the ſpirits within the tube (which I apprehended it might do on account of the ſuperiority of its ſpecific gravity, eſpecially when kept for a conſiderable time, very high on one ſide, and low on the other), I made that part of the ſmall tube from *e* to *f* with the inner diameter exceeding ſmall; and found, upon trial, that after the ſummer's heat had kept the mercury for a long time high on one ſide, the winter's cold brought it again as accurately to the freezing point on the other as at firſt*. This thermometer may be made a mercurial one by inverting the glaſs, and filling with mercury that part which in the firſt is filled with ſpirits, and with ſpirits that part of the ſmall tube from *d* to *g* which in the former is filled with mercury; the indexes in either caſe may be the ſame, and will be carried up in the ſame manner upon the ſurface of the mercury; but the end of the tube at *i*, inſtead of being ſealed, muſt then be left open, and ſtand inverted in a bulb, or ſmall ciſtern of mercury, into which the external air has free acceſs. The diameter of the tube *a b* ſhould be conſiderably increaſed if the degrees on the ſcale are required to be as wide as thoſe in the ſpirit thermometers. It is indeed better in this caſe to have a double rather than a larger ſingle tube; but finding the weight of ſo great a quantity of mercury in a thin glaſs tube

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[*] Phil. Tranſ. vol. L. p. 501. and vol. LI. p. 820.

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* The diviſions below the freezing point are taken by means of a mixture of ſea ſalt and ice, as deſcribed by NOLLET, DE LUC, and others.

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* When this tube is cloſed (not hermetically, but only ſo as to prevent the ſpirits evaporating) the thermometer muſt be brought to the greateſt heat it is likely at any time after to ſuſtain; and though no more air is incloſed than what remains at that time above the ſpirits, yet that will, by its elaſticity preſſing on the fluid, anſwer every purpoſe as well as if the external air was freely admitted.

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* With a thermometer of this ſort I obſerved the greateſt heat and cold that happened every day and night throughout the year 1781.

attended with many diſadvantages, and the motion of the fluids in the ſpirit-ones perfectly agreeing with, and being as readily excited by change of heat and cold, as in the mercurial thermometers, I preferred the former as much more commodious. A perſon cannot approach near to the thermometer firſt deſcribed when the air is very cold (eſpecially with a light which by night is neceſſary) without cauſing the ſpirits preſently to expand, and conſequently the mercury on the left ſide immediately to deſcend. This ſenſibility is here attended with every advantage, without the inconvenience to which common thermometers in this caſe are liable *; for the index will accurately ſhew the greateſt height to which the mercury had riſen, although, before the exact degree can well be diſtinguiſhed, it will appear ſeparated from the index, and deſcending apace. As the ſcale is ſixteen inches long, and divided into 100° only, which are more than ſufficient for the temperature of the air, they are large enough to be ſub-divided at pleaſure. The indexes, though of a tender and delicate nature, when once placed in the tube, are not liable to ſuffer any alteration by time or accident; and the thermometer may be expoſed to rain at all times, without ſuffering the leaſt injury in any reſpect.

In conſtructing the thermometer before mentioned, I at firſt hit on a plan by which the ſame end was obtained by a dif-

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* The moſt ſenſible mercurial thermometers commonly have the column of mercury as well as the degrees very ſmall, and a perſon aſſiſted with a light can hardly view them near enough, when the weather is very cold, without cauſing the mercury to riſe before the degrees where it ſtood can be well aſcertained.

Freezing fogs alſo, which with us uſually attend the greateſt degrees of cold, by covering the glaſs with froſt, render the mercury inviſible, and cannot well be mercury removed without cauſing the to riſe, or at leaſt render the obſervation doubtful, which at ſuch a time is very diſagreeable; for, in proportion to the extraordinary degree of cold, ſo is our curioſity likely to be excited.

ferent method; and though, in ſome reſpects, and for ſome purpoſes, it may not be ſo proper as that already deſcribed, yet, for ſome others, it may be found uſeful, and therefore I ſhall briefly deſcribe it. The glaſs of this inſtrument is in all reſpects the ſame as in the former, excepting that the diameters of the tubes are ſomething larger. It is likewiſe filled with ſpirits of wine and mercury, in the ſame manner; but the indexes are different, being only a ſmall tube of black glaſs, about five-ſevenths of an inch in length, hermetically ſealed at each end, containing a piece of ſteel wire. An index of this ſort is placed in the thermometer on either ſide, which, having no ſpring to ſupport them, ſink down in the ſpirits, and reſt upon the mercury. Whenever the mercury deſcends, the index will follow it; but when it riſes, the index will not riſe with it, and by remaining at the place to which the mercury had deſcended, will ſhew the greateſt degree of heat or cold which had happened. In this manner do theſe indexes anſwer the ſame purpoſe, though they move directly contrary to the others in the other thermometer; but this inſtrument is not ſo eaſily rectified as the former, for the moſt powerful magnet will not bring the index up again while the mercury above preſſes againſt them; and although it is poſſible to remove the mercury, and by that means ſet the index at liberty, yet inconveniences will be incurred from which the other is entirely free.

In ſome caſes it may be found expedient, inſtead of the double thermometer firſt deſcribed, to make two ſingle ones; one to ſhew the greateſt degree of heat only, and the other the cold, each having its proper index (ſee fig. 4. and 5.). The firſt has the ſmall tube bent down on the left ſide, and the lower end immerſed in a bulb or ſmall ciſtern of mercury, to which the external air has free acceſs; the other has the ſmall tube turned up on the right ſide, with ſome mercury let down to the bottom, and the upper end cloſely ſealed, as in the double inſtrument. Making a ſtandard mercurial thermometer, by which the ſcale of the ſpirit one was to be divided, I endeavoured to obtain as wide degrees as poſſible, that the motion of the mercury might thereby be rendered more conſpicuous, and the height of it aſcertained with greater preciſion. It is true, the larger the degrees, the larger in ſome meaſure muſt be the bulb, and therefore the fluid contained in it not likely to be ſo ſoon affected by any change of heat or cold in the atmoſphere as in a ſmaller. But as this thermometer was principally to be uſed immerſed in a large quantity of water, gradually heating or cooling, little or no diſadvantage could ariſe from making the bulb ſomewhat larger than thoſe commonly made uſe of in the air. Not being able, however, to procure glaſs tubes ſo long as I had occaſion for, whoſe inner diameters were perfectly equal, I took the following method to adjuſt the diviſions on the ſcale to the inequality of the tubes. Chooſing a tube of a length ſuitable to my purpoſe, with a proper bulb at the end, I put into it a ſmall quantity of mercury * ſufficient to form a column about one inch in length. Drawing then on a board the three lines *aa*, *bb*, *cc*, fig. 6. I placed the glaſs tube on the line *aa*, and while the mercury remained at reſt at the end of the tube, near the bulb, I made two pencil marks on the line *aa*, one at *d*, and the other at *e*, perfectly coinciding with the two ends of the column of mercury: then cauſing the mercury to move ſlowly on farther from the bulb, till that end of the column which was firſt at *d* coincided with the mark at *e*, and letting it reſt again, I made another mark at *f*; after which, cauſing the mercury to move on as before, and continuing to mark its length at every part of the tube till it reached the end fartheſt from the bulb; by theſe means I obtained the ſeveral intermediate points on the line *aa*. Through theſe ſeveral points I drew dotted lines parallel to each other, and at right angles with the line *aa* to the line *bb*. Taking now, with a pair of compaſſes, the wideſt intervals between any of the dotted parallels, which in this caſe is from *d* to *e*, I inſerted that diſtance ſucceſſively between the ſeveral parallels, beginning at the loweſt pair, as from *d* to *e*, from *e* to *f*, from *f* to *g*, and ſo on to *h*, as exhibited in the figure; and the aggregate of theſe lines may be conſidered as one continued line, without any error of conſequence in this matter. Having now the thermometer completely filled with mercury, the air expelled, the point of the ſcale at 102°, and the freezing point properly taken * and marked upon the tube, which was now hermetically ſealed, I again applied the tube to the line *aa*, and marked on that line the point of 102° and the freezing point. Through thoſe points I drew the lines *ii*, *kk*, and divided that part of the compound line *dh* included between *ii* and

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* To put in a ſmall quantity of mercury, and meaſure its length at different parts of the tube, as deſcribed by Abbé NOLLET, vol. IV. p. 370. Lecons Phyſique, is a very excellent method to diſcover the error; but in what manner readily to adjuſt the ſcale, ſo as to avoid any inaccuracy from ſuch inequality (which in tubes of the length I had occaſion for ſeemed to me unavoidable) was a matter concerning which I could meet with no information.

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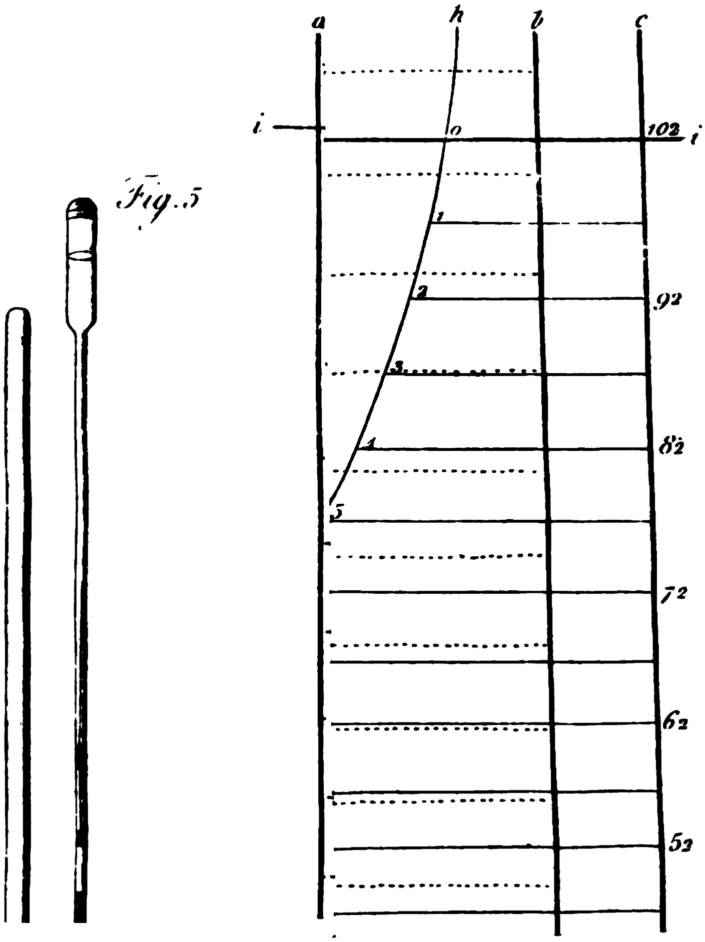
* The freezing point, marked on the tube of this thermometer, is immediately taken by means of grated ice; but the point of 100° by a ſtandard mercurial thermometer, the upper point of the ſcale of which was properly taken by boiling water, and the lower one by grated ice; but it is more commodious in the firſt to have the tube no longer than the air ſcale, eſpecially as the degrees are pretty wide. The method of adjuſting the ſcale to the inequality of the tube remains the ſame, let the given points be at any diſtance, or the diviſions increaſed to any number.

Fig. 5



and *kk* into 14 equal parts, beginning at *o*, the point where *ii* cuts the line *db*, continuing afterward ſix diviſions now on that line below *kk*, making in all 20 equal diviſions. If now lines be drawn through each of the dividing points, from *o* to 20 to the line *cc*, at right angles with the ſame, they will give on the line *cc* the true thermometrical ſcale to every 5° from 2 to 102, properly adjuſted to the inequality of the tube *, which in this caſe is nearly of the ſame diameter at each end, but ſmaller towards the middle. Tubes may indeed be found of ſome conſiderable length with leſs inequality than what this ſcale exhibits; but the error is here enlarged, to render the method of correcting it more conſpicuous.

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* Experimentally to prove this method I have made mercurial thermometers, whoſe ſcales from the freezing point to that of boiling heat were nearly three feet, and though the inequalities of the tubes were very conſiderable, varying in contrary directions to each other; yet when they were placed on the ſame frame, they perfectly agreed in a motion of the mercury in every part of their ſcales.

XI. *On the Parallax of the Fixed Stars. By Mr.* Herſchel, *F. R. S.; communicated by Sir* Joſeph Banks, *Bart. P. R. S.*

Read December 6, 1781.

TO find the diſtance of the fixed ſtars has been a problem which many eminent aſtronomers have attempted to ſolve; but about which, after all, we remain in a great meaſure ſtill in the dark. Various methods have been purſued without ſucceſs, and the reſult of the fineſt obſervations has hardly given us more than a diſtant approximation, from which we may conclude, that the neareſt of the fixed ſtars cannot be leſs than forty thouſand diameters of the whole annual orbit of the earth diſtant from us. Trigonometry, by whoſe powerful aſſiſtance the mathematician has boldly aſcended into the planetary regions, and meaſured the diameters and orbits of the heavenly bodies, for want of a proper baſe, can here be but of little ſervice; for the whole diameter of the annual orbit of the earth is a mere point when compared to the immenſe diſtance of the ſtars. Now, as it is not in our power to enlarge this baſe, we can only endeavour to improve the inſtruments by which we meaſure its parallax.

There are two things requiſite for meaſuring extremely ſmall angles with accuracy. Firſt, that the inſtrument we uſe for this purpoſe, be it quadrant, ſector, or micrometer, ſhould be divided and executed with ſufficient exactneſs; and, ſecondly, that the teleſcope, by which the obſervations are to be made, ſhould have an adequate power and diſtinctneſs. Upon the firſt head, the great improvements of mathematical inſtrument-makers have hardly left us any thing to deſire: we can now meaſure ſeconds with almoſt as much facility and truth as former obſervers could meaſure minutes; nor do I think it impoſſible to go ſtill further, and divide inſtruments that would ſhew thirds with ſufficient accuracy. It is in the latter, or optical part, we find the greateſt difficulty. To ſee a ſingle ſecond of a degree with preciſion requires a teleſcope of very great perfection; therefore, ſuppoſing the mechanical part of an apparatus well executed, it will ſtill be neceſſary to try how far the power of our teleſcope will enable us to aſcertain with confidence the diviſion or number of ſeconds it points out. If upon trial we find that our inſtrument will give us the ſame meaſure within the ſecond, every time the experiment is repeated, we may pronounce it capable of meaſuring ſeconds; if otherwiſe, it will remain to be examined, whether the fault lies in the mechanical or optical part.

Let us now ſuppoſe that the parallax of the fixed ſtars does not amount to a ſingle ſecond, yet ſtill the caſe is by no means deſperate; and though the difficulty of meaſuring ſeconds will ſoon ſuggeſt to us what extraordinary powers and diſtinctneſs of the teleſcope, and accuracy of the micrometer, are required to meaſure thirds; this ought by no means to diſcourage us in the attempt. Could we meaſure angles, much ſmaller than ſeconds, might we not hope to find the parallax of ſome of the fixed ſtars at leaſt to amount to ſeveral thirds? On the other hand, if it ſhould appear, indeed, that even with ſuch improved methods of meaſurement we could not reach the remote ſituation of ſuch almoſt infinitely diſtant ſuns, we might ſtill derive a valuable approximation towards truth from ſuch repeated obſervations, even though they ſhould not be attended with all the ſucceſs we expected from them. On this aſſurance, I endeavoured to take ſuch a method for attempting the inveſtigation of the parallax of the ſtars as to avail myſelf of the improvements I had already made, and was ſtill in hopes of making, in my teleſcopes.

The next thing that was neceſſary to conſider in this undertaking was, the manner of putting it into execution. The method pointed out by GALILEO, and firſt attempted by HOOK, FLAMSTEAD, MOLINEUX, and BRADLEY, of taking diſtances of ſtars from the zenith that paſs very near it, though it failed with regard to parallax, has been productive of the moſt noble diſcoveries of another nature. At the ſame time it has given us a much juſter idea of the immenſe diſtance of the ſtars, and furniſhed us with an approximation to the knowledge of their parallax that is much nearer the truth than we ever had before. Dr. BRADLEY, in a letter to Dr. HALLEY on the ſubject of a new discovered motion of the fixed ſtars, ſays, "I believe I may venture to ſay, that in either of the "two ſtars laſt mentioned (γ Draconis and η Urſæ majoris) it "(the annual parallax) does not amount to 2″. I am of opi- "nion, that if it were 1″ I ſhould have perceived it in the "great number of obſervations that I made, eſpecially upon "γ Draconis; which agreeing with the hypotheſis (without "allowing any thing for parallax) nearly as well when the "ſun was in conjunction with, as in oppoſition to, this ſtar, "it ſeems very probable, that the parallax of it is not ſo great "as one ſingle ſecond." Phil. Tranſ. n. 406. p. 637. Dec. 1728. As I do not know that any thing more deciſive has been done upon the ſubject, it will not be amiſs to ſee how far this method of finding the parallax has really been ſucceſsful.

The inſtrument that was uſed upon this occaſion was the ſame as the preſent zenith ſectors, which can hardly be allowed ſufficient to ſhew an angle of one or even two ſeconds with accuracy; yet, on account of the great number of obſervations, and above all the great ſagacity of the obſerver, we will admit that if the parallax had amounted to two ſeconds he would have perceived it. The ſtar on which theſe obſervations were made is marked of the third magnitude in the catalogue of PTOLEMY; in TYCHO BRAHE's of the third; in the Prince of HESSE's of the third; in HEVELIUS's between the third and ſecond; in FLAMSTEAD's of the ſecond; and now appears as a very bright ſtar of the third, or ſmall ſtar of the ſecond magnitude; therefore its parallax is probably conſiderably leſs than that of a ſtar of the firſt magnitude. Several authors who have touched upon this ſubject ſeem to have overlooked this diſtinction; and from Dr. BRADLEY's account of the parallax of γ Draconis, have concluded the parallax of the ſtars in general not to exceed 1″; but this appears to me by no means to follow from the doctor's obſervations. It is rather evident that, for aught we know to the contrary, the ſtars of the firſt magnitude may ſtill have a parallax of ſeveral ſeconds; and I believe this to be as accurate a reſult as that method is capable of giving, at leaſt in latitudes where there is not a ſtar of the firſt magnitude that paſſes directly through the zenith *.

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* DE LA LANDE, in his excellent book of Aſtronomy, ſays, that the parallax of the fixed ſtars has been proved to be abſolutely inſenſible (Aſt. liv. XVI. § 2782.). He reports the obſervations of TYCHO BRAHE, PICARD, HOOK, and FLAMSTEAD, and concludes (§ 2778.) from the diſcovery of the aberration by Dr. BRADLEY (which it ſeems he alſo allows to be the moſt deciſive upon the ſubject) that now the queſtion about parallax is reſolved. In giving us the opinion which

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In general, the method of zenith diſtances labours under the following conſiderable difficulties. In the firſt place, all theſe diſtances, though they ſhould not exceed a few degrees, are liable to refractions; and I hope to be pardoned when I ſay that the real quantities of theſe refractions, and their differences,

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the doctor had of the reſult of his own obſervations with regard to the annual parallax, DE LA LANDE only mentions “M. BRADLEY penſe que ſi elle (la “parallaxe) eût été ſeulement de 1″ il l'auroit apperçue dans le grand “nombre d'obſervations qu'il avoit faites, ſurtout de γ du Dragon.” But if we alſo take in thoſe lines upon which Dr. BRADLEY ſeems to lay the greateſt ſtreſs, viz. “I believe I may venture to ſay, that in either of the “two ſtars laſt mentioned it does not amount to two ſeconds;” and if we allow for the magnitude of the ſtars upon which the obſervations were made, I think I have fairly ſtated the full amount of all the actual proofs we have of the ſmallneſs of the annual parallax. Now, ſince it has eſcaped the fineſt obſervations of BRADLEY, it is not likely that it ſhould come up to the full quantity to which it might amount without being perceived; and therefore the doctor might think it highly probable, “that it is not ſo great as one ſingle ſecond;” and his opinion, as well as DE LA LANDE's, who believes it to be abſolutely inſenſible, are perfectly conſiſtent with all the obſervations that have hitherto been made; though the *actual proofs*, which are the ſubject of our preſent inquiry, do not extend ſo far. Againſt the parallax of Sirius DE LA LANDE (§ 2781.) mentions “forty-five meridian altitudes taken by Dr. BEVIS [a], with the eight-feet mural quadrant of the Royal Obſervatory at Greenwich, none of which differed 3 or 4″ “from the mean altitude.” Now, if they differed 3 or 4″ from the mean, we may ſuppoſe they differed 6 or 8″ from each other; and that obſervations, ſubject to ſo many cauſes of error as I ſhall preſently enumerate, and which differed ſo much from each other, cannot give the leaſt evidence either for or againſt a parallax, will need no proof. Refraction alone, which is liable to ſuch changes at the meridian altitude of Sirius, notwithſtanding the moſt careful obſervations of the barometer and thermometer ſhould be made to aſcertain its quantity, would, with me, remain an unanſwerable argument againſt the validity of ſuch obſervations in a ſubject of this critical nicety.

[a] Theſe obſervations were not made by Dr. BEVIS, but extracted from the regiſters of the Royal Obſervatory at my deſire, and calculated by myſelf, and ſent in a letter by Dr. BEVIS to Paris.

NEVIL MASKELYNE.

are very far from being perfectly known. Secondly, the change of poſition of the earth's axis ariſing from nutation, preceſſion of the equinoxes, and other cauſes, is ſo far from being completely ſettled, that it would not be very eaſy to ſay what it exactly is at any given time. In the third place, the aberration of light, though beſt known of all, may alſo be liable to ſome ſmall errors, ſince the obſervations from which it was deduced laboured under all the foregoing difficulties. I do not mean to ſay, that our theories of all theſe cauſes of error are defective; on the contrary, I grant that we are for moſt aſtronomical purpoſes ſufficiently furniſhed with excellent tables to correct our obſervations from the above mentioned errors. But when we are upon ſo delicate a point as the parallax of the ſtars; when we are inveſtigating angles that may, perhaps, not amount to a ſingle ſecond, we muſt endeavour to keep clear of every poſſibility of being involved in uncertainties; even the hundredth part of a ſecond becomes a quantity to be taken into conſideration.

I ſhall now deliver the method I have taken, and ſhew that it is free from every error to which the former is liable, and is ſtill capable of every improvement the teleſcope and mechaniſm of micrometers can furniſh.

Let OE (fig. 1.) be two oppoſite points of the annual orbit, taken in the ſame plane with two ſtars *a*, *b*, of unequal magnitudes. Let the angle *a*O*b* be obſerved when the earth is at O: and let the angle *a*E*b* be alſo obſerved when the earth is at E. From the difference of theſe angles, if any ſhould be found, we may calculate the parallax of the ſtars, according to a theory that will be delivered hereafter. Theſe two ſtars, for reaſons that will ſoon appear, ought to be as near each other as poſſible, and alſo to differ as much in magnitude as we can find them.

GALILEO, I believe, was the firſt who ſuggeſted this method; but in the manner he mentions it in his third dialogue of the *Syſtema Coſmicum*, it would be expoſed to all the difficulties we have enumerated, and would wiſh to avoid; for he does not obſerve, that the two ſtars ſhould be ſo near each other as thereby to preclude the influence of every cauſe of error.

This method has alſo been mentioned by other authors; and we find that Dr. LONG obſerved the double ſtar which is the firſt of Aries in PTOLEMY's catalogue; that in the head of Caſtor; the middle one in the ſword of Orion; and that in the breaſt of Virgo, with teleſcopes of fourteen and ſeventeen feet, and " was perſuaded they would be found always to appear " the ſame." But when the theory of parallax will be explained, it will be ſeen that every one of theſe ſtars are totally improper for the purpoſe; for the ſtars of γ Arietis are near 10″ diſtant from each other, and moreover equal in magnitude. In α Geminorum the ſtars, though near enough, do not ſufficiently differ in magnitude to ſhew any parallax. The ſtars in the Nebula of Orion, on account of their extreme ſmallneſs or diſtance, are ſtill more improper than any; and thoſe of γ Virginis are equal in magnitude.

I do not find that any thing elſe has been done upon the ſubject. GALILEO juſtly remarks, that ſuch obſervations ought to be made with the beſt teleſcopes, and upon this occaſion mentions the power of his own, which enlarged the diſk of the ſun a thouſand times, from which we find it magnified about thirty-two times; but we can hardly think his nor even Dr. LONG's, whoſe power might probably be ſixty or ſeventy, ſufficient for the purpoſe. What would GALILEO ſay, if he were told that our preſent opticians make inſtruments that enlarge the diſk of the ſun above forty thouſand times? What would even CASSINI ſay, if he were to view the firſt ſtar of Aries, which appeared to him as ſplit in two, through a teleſcope that will ſhew η Coronæ borealis and *b* Draconis to be double ſtars?

But to proceed, I ſhall now prove that this method, if ſtars properly ſituated (ſuch as I have found) are taken, is free from all the errors occaſioned by refraction, nutation, preceſſion of the equinoxes, changes of the obliquity of the ecliptic, and aberration of light; and that the annual parallax, if it even ſhould not exceed the tenth part of a ſecond, may ſtill become viſible, and be aſcertained at leaſt to a much greater degree of approximation than it ever has been done.

It will alſo appear, from the great number of obſervations I have already made upon ſeveral double ſtars, eſpecially ε Bootis, that we can now with much greater certainty affirm the annual parallax to be exceedingly ſmall indeed; and that there is a great probability of ſucceeding ſtill farther in this laborious but delightful reſearch, ſo as to be able at laſt to ſay, not only how much the annual parallax *is not*, but how much it really *is*.

Let there be two ſtars at a diſtance from each other, not exceeding five ſeconds; ſuppoſe them to be obſerved at an altitude of 20°; and let them be ſo ſituated with reſpect to each other, that one of them may be 20°, and the other 20° and 5″ high: then the whole effect of mean refraction at that altitude, by Dr. MASKELYNE's excellent tables, will be 2′ 35″,5 for 20°, and 2′ 35″,4888 for 20° 5″. The difference is 0″,0111. Now, in the firſt place, we have nothing to do with the refraction itſelf, ſince the real altitude of the ſtars is not in queſtion. In the next place, we alſo have no concern with the difference of refrac-

and *kk* into 14 equal parts, beginning at *o*, the point where *ii* cuts the line *db*, continuing afterward ſix diviſions now on that line below *kk*, making in all 20 equal diviſions. If now lines be drawn through each of the dividing points, from *o* to 20 to the line *cc*, at right angles with the ſame, they will give on the line *cc* the true thermometrical ſcale to every 5° from 2 to 102, properly adjuſted to the inequality of the tube *, which in this caſe is nearly of the ſame diameter at each end, but ſmaller towards the middle. Tubes may indeed be found of ſome conſiderable length with leſs inequality than what this ſcale exhibits; but the error is here enlarged, to render the method of correcting it more conſpicuous.

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* Experimentally to prove this method I have made mercurial thermometers, whoſe ſcales from the freezing point to that of boiling heat were nearly three feet, and though the inequalities of the tubes were very conſiderable, varying in contrary directions to each other; yet when they were placed on the ſame frame, they perfectly agreed in a motion of the mercury in every part of their ſcales.

does not appear. Theories are only to be uſed when proper data are aſſigned; but the data are carefully to be re-examined, when new improvements may widely alter the reſult of former experiments. Thus, we are told, that we gain nothing by magnifying *too much*. I grant it; but ſhall never believe I magnify too much till by experience I find, that I can ſee better with a lower power. Nor is even that ſufficient: a lower power may ſhew more of the object; it may ſhew it brighter, nay even diſtincter, and therefore upon the whole better; and yet the greater power may, in a particular caſe, be preferable: for if the object is ſo ſmall as not to be at all viſible with the lower power, and I can, by magnifying more, obtain a view of it, though neither ſo bright nor diſtinct as I could wiſh, is it not evident, that here this power is preferable to the former?

The naturaliſt does not think himſelf obliged to account for all the phænomena he may obſerve; the aſtronomer and optician may claim the ſame privilege. When we increaſe the power we leſſen the light in the inverſe ratio of the ſquare of the power; and teleſcopes will, in general, diſcover more ſmall ſtars the more light they collect; yet with a power of 227 I cannot ſee the ſmall ſtar near the ſtar following *o* Aquilæ, when, by the ſame teleſcope, it appears very plainly with the power of 460: now, in the latter caſe, the power being more than double, the light is leſs than the fourth part of the former. In ſuch particular caſes I generally ſuſpect my own eyes, and have recourſe to thoſe of my friends. I had the pleaſure of ſhewing this ſtar to Dr. WATSON junior, who ſoon diſcovered the ſmall ſtar, which accompanies the other, with the power of 460; but ſaw nothing of it with 227, though the place where to look for it had been pointed out to him by the higher power. The experiment has been too often repeated to be doubtful, and has alſo been confirmed by others of nearly the ſame nature: for inſtance, the ſmalleſt of the two that accompany the ſtar near *k* Aquilæ, the ſmall ſtar near *μ* Herculis, and the ſmall ſtar near *α* Lyræ, are inviſible with my power of 227, and viſible with the ſame aperture when the power is 460. Alſo the ſmall ſtars near FLAMSTEAD's 24th of Aquila, the ſmalleſt of two near *σ* Coronæ, the ſmall ſtar near the ſtar ſouth of *ε* Aquilæ, the ſmall ſtar near the ſecond *o* Perſei, the ſmall ſtar near the ſtar which accompanies FLAMSTEAD's 10th ſub pede et ſcapula dextra Tauri, the ſmall ſtar, near β Delphini, and the ſmall ſtar near the pole ſtar, are all much brighter and ſtronger, and therefore much ſooner ſeen with 460 than with 227.

Great power may alſo, in particular circumſtances, be favourable, even with an exceſs of aberration. When two ſtars are ſo cloſe together as to make the ſcale for meaſuring the diſtance of their centers too ſmall, if, by magnifying much, we can enlarge that diſtance, we may gain a conſiderable advantage, provided the centers or apparent bodies of the ſtars remain diſtinct enough for the purpoſe of theſe meaſures. The appearance of *α* Lyræ in my Newtonian reflector with a power of 460 is repreſented in fig. 2.; with 2010 in fig. 3.; with 3168 in fig. 4.; and with 6450 in fig. 5. Now in all theſe figures we ſee, that the centers are ſtill diſtinct enough to meaſure their diſtances with ſufficient truth; or if any little error ſhould be introduced by the magnitude of the central point, it will be more than ſufficiently balanced by the largeneſs of the ſcale. In this manner, with a power of 3168, I have obtained a ſcale of no leſs than ten inches ſix tenths for the diſtance of the centers of the two ſtars of *α* Geminorum; and as we know theſe centers to be but a few ſeconds diſtant, it is plain how great an advantage we gain by ſuch an enlarged ſcale.

Theſe experiments have but very lately pointed out to me a method of making a new micrometer, upon a conſtruction entirely different from any that are now in uſe, which I have been ſucceſsful enough to put in practice, and by which I have already begun to determine the diſtance of the centers of ſome of the moſt remarkable double ſtars to a very great degree of accuracy *.

The powers that may be uſed upon various double ſtars are different, according to their relative magnitudes: ε Bootis, for inſtance, will not bear the ſame power as α Geminorum, nor would it be difficult to aſſign a reaſon for it; but as I here ſhall merely confine myſelf to facts, it will be ſufficient in general to mention, that two ſtars, which are equal, or nearly ſo, will bear a very high power: with α Geminorum I have gone as far as 3168; but with the former only to 2010. The difficulty of uſing high powers is exceedingly great; for the field of view takes in leſs than the diameter of the hair or wire in the finder, and the effect of the earth's diurnal motion is ſo great, that it requires a great deal of practice to find the object, and manage the inſtrument. It appears to me very probable, that the diurnal motion of the earth will be the greateſt obſtacle to our progreſs in magnifying, except we can introduce a proper mechaniſm to carry our teleſcopes in a contrary motion.

Notwithſtanding opticians have proved that two eye-glaſſes will give a more correct image than one, I have always (from experience) perſiſted in refuſing the aſſiſtance of a ſecond glaſs, which is ſure to introduce errors greater than thoſe we would correct. Let us reſign the double eye-glaſs to thoſe who view objects

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* For a deſcription of this micrometer ſee a ſubſequent paper.

merely for entertainment, and muſt have an exorbitant field of view. To a philoſopher this is an unpardonable indulgence. I have tried both the ſingle and double eye-glaſs of equal powers, and always found that the ſingle eye-glaſs had much the ſuperiority in point of light and diſtinctneſs. With the double eye-glaſs I could not ſee the *belts on Saturn*, which I very plainly ſaw with the ſingle one. I would, however, except all thoſe caſes where a large field is abſolutely neceſſary; and where power joined to diſtinctneſs is not the ſole object of our view.

The application of the different powers of a teleſcope in general is of ſome conſequence; and in anſwer to thoſe who may think I have ſtrained or over-charged mine, I muſt obſerve; that a ſingle glance at the ſubſequent *b* Draconis, τ Coronæ; and the ſtar near μ Bootis, with a power of 460, ſhewed them to me as double ſtars; when, in two former reviews of the heavens, I had twice ſet them down in my journal as ſingle ſtars, where I uſed only the power of 222 and 227, and in all probability ſhould never have found them double, had I not looked with a higher power.

We are to remember, that it is much eaſier to ſee an object when it is pointed out to us than when it falls in our way unexpectedly, eſpecially if of ſuch a nature as to require ſome attention to be ſeen at all; but to ſay no more of other advantages of high powers, it is evident, that in the reſearch of the parallax of the fixed ſtars they are abſolutely neceſſary. If we would diſtinctly perceive and meaſure or eſtimate extremely ſmall quantities, ſuch as a tenth of a ſecond, it appears, that when we uſe a power of 460, this tenth of a ſecond will be no more in appearance than 46″, and even with a power of 1500 will be but 2′ 30″, which is a quantity not much more than ſufficient to judge well of objects and diſtinguiſh them from each other, ſuch as a circle from a ſquare, triangle, or polygon *.

It has been obſerved, that objects grow indiſtinct when the principal optic pencil at the eye becomes leſs than the 40th or 50th part of an inch in diameter. In the experiments that have been made upon this ſubject it appears to me, that the indiſtinctneſs which is aſcribed to the ſmallneſs of the optical pencil may be owing to very different cauſes: at leaſt it will be eaſy to bring contrary experiments of extremely ſmall pencils, not at all affected by this inconvenience; for inſtance, it is well known, that microſcopes, conſiſting of a ſingle lens or globule, are remarkable for diſtinctneſs. We alſo know, that they have been made ſo ſmall as to magnify above 10,000 times †. From this we may infer that their apertures, and conſequently the diameters of the optic pencil at the eye could not exceed the 2500th part of an inch. I am therefore inclined to believe, that we muſt look for diſtinctneſs in the perfection of the object-ſpeculum or object-glaſs of a teleſcope; and if we can make the firſt image in the focus of a ſpeculum almoſt as perfect as the real object, what ſhould hinder our magnifying but the want of light? Now, if the object has light ſufficient, as the ſtars moſt undoubtedly have, I ſee no reaſon why we ſhould limit the powers of our inſtruments by any theory. Is it not beſt to have recourſe to experiments to find

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* By a ſet of experiments, made in the year 1774, I found, that I could diſcover or perceive a bright object, ſuch as white paper, againſt the ſky-light, when it ſubtended an angle of 35″; but could only diſtinguiſh it to be a circle, and no other figure, when it appeared under an angle of 2′ 24″.

† See Padre DELLA TORRE'S Method, &c. Scelta di Opuſculi.

how far our endeavours to render the firſt image perfect have been ſucceſsful.

As ſoon as I was fully ſatisfied that in the inveſtigation of parallax the method of double ſtars would have many advantages above any other, it became neceſſary to look out for proper ſtars. This introduced a new ſeries of obſervations. I reſolved to examine every ſtar in the heavens with the utmoſt attention and a very high power, that I might collect ſuch materials for this reſearch as would enable me to fix my obſervations upon thoſe that would beſt anſwer my end. The ſubject has already proved ſo extenſive, and ſtill promiſes ſo rich a harveſt to thoſe who are inclined to be diligent in the purſuit, that I cannot help inviting every lover of aſtronomy to join with me in obſervations that muſt inevitably lead to new diſcoveries. I took ſome pains to find out what double ſtars had been recorded by aſtronomers; but my ſituation permitted me not to conſult extenſive libraries, nor indeed was it very material: for as I intended to view the heavens myſelf, Nature, that great volume, appeared to me to contain the beſt catalogue upon this occaſion. However, I remembered that the ſtar in the head of Caſtor, that in the breaſt of the Virgin, and the firſt ſtar in Aries, had been mentioned by CASSINI as double ſtars. I alſo found the Nebula in Orion was marked in HUGEN's *Syſtema Saturnium* as containing ſeven ſtars, three of which (now known to be four) are very near together. With this ſmall ſtock I begun, and in the courſe of a few years obſervations have collected the ſtars contained in my catalogue. I find, with great pleaſure, that a very excellent obſerver, whom I have the honour to call my friend*, has alſo, though unknown to me, met with three of thoſe ſtars that will be found in my catalogue: and upon this occaſion I alſo beg leave to obſerve, that the Aſtronomer Royal, when I was at Greenwich laſt May, with his uſual politeneſs, ſhewed me, among other objects, α Herculis as a double ſtar, which he had diſcovered ſome years ago. The rev. Mr. HORNSBY alſo, when I had the pleaſure of ſeeing him at Oxford, in a converſation on the ſubject of the ſtars of the firſt magnitude that have a proper motion, mentioned π Bootis as a double ſtar. It is a little hard upon young aſtronomers to be obliged to diſcover *over-again* what has already been diſcovered; however, the pleaſure that attended the view when I firſt ſaw theſe ſtars has made ſome amends for not knowing they had been ſeen before me.

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* Phil. Tranſ. for the year 1781, part II. double ſtars diſcovered in 1779, at Frampton-houſe, Glamorganſhire, by NAT. PIGOTT, Eſq. F. R. S. &c.

If I ſhould mention in my liſt of obſervations a few that may be found difficult to be verified by other teleſcopes, I muſt beg the indulgence of the obſervers. I hope it will ſufficiently appear, that I have guarded againſt optical deluſions; and every aſtronomer, I make no doubt, will find, by thoſe obſervations that fall within the compaſs of his inſtruments, and attention to circumſtances neceſſary to the right management of them, that I have had all along truth and reality in view, as the ſole object of my endeavours; and therefore he will be inclined to give ſome credit to what he does not immediately perceive, when he finds himſelf ſucceſsful where he takes the proper precautions ſo neceſſary in delicate obſervations, even with the beſt inſtruments.

I have been in ſome doubt in what manner to communicate theſe obſervations. My firſt view was to have methodized them properly; but I find them ſo extenſive that there is but little probability that one perſon ſhould be able to bring them to a concluſion, for which reaſon I have now reſolved to give them unfiniſhed as they are, that every perſon who is inclined to engage in this purſuit may become a fellow-labourer.

In ſettling the diſtances of double ſtars I have occaſionally uſed two different ways. Thoſe that are extremely near each other may be eſtimated by the eye, in meaſures of their own apparent diameters. For this purpoſe their diſtance ſhould not much exceed two diameters of the largeſt, as the eye cannot ſo well make a good eſtimation when the interval between them is greater. This method has often the preference to that of the micrometer: for inſtance, when the diameter of a ſmall ſtar, perhaps not equal to half a ſecond, is double the vacancy between the two ſtars. Here a micrometer ought to meaſure tenths of ſeconds at leaſt, otherwiſe we could not, with any degree of confidence, rely on its meaſures; nay, even then, if the ſtars are ſituated in the ſame parallel of declination and near the equator, their quick motion acroſs the micrometer makes it extremely difficult to meaſure them, and in that caſe an eſtimation by the eye is preferable to any other meaſure; but this requires not a little practice, precaution, and time, and yet with proper care it will be found that this method is capable of great exactneſs. Let two ſmall circles be drawn either equal or unequal, at a diſtance not exceeding twice the diameter of the largeſt; let theſe be ſhewn to ſeveral perſons in the ſame light and point of view. Then, if every one of them will ſeparately and carefully write down his eſtimation of the interval between them, in the proportion of either of their diameters, it will be found upon a compariſon that there will ſeldom be ſo much as a quarter of a diameter difference between all the eſtimations. If this agreement takes place with ſo many different eyes, much more may we expect it in the eſtimations of the ſame eye when accuſtomed to this kind of judgement.

I have divided the double ſtars into ſeveral different claſſes. In the firſt I have placed all thoſe which require indeed a very ſuperior teleſcope, the utmoſt clearneſs of air, and every other favourable circumſtance to be ſeen at all, or well enough to judge of them. They ſeemed to me on that account to deſerve a ſeparate place, that an obſerver might not condemn his inſtrument or his eye if he ſhould not be ſucceſsful in diſtinguiſhing them.

As theſe are ſome of the fineſt, moſt minute, and moſt delicate objects of viſion I ever beheld, I ſhall be happy to hear that my obſervations have been verified by other perſons, which I make no doubt the curious in aſtronomy will ſoon undertake. I ſhould obſerve, that ſince it will require no common ſtretch of power and diſtinctneſs to ſee theſe double ſtars, it will therefore not be amiſs to go gradually through a few preparatory ſteps of viſion, ſuch as the following: when η Coronæ borealis (one of the moſt minute double ſtars) is propoſed to be viewed, let the teleſcope be ſome time before directed to α Geminorum, or if not in view to either of the following ſtars, ζ Aquarii, μ Draconis, ρ Herculis, α Piſcium, or the curious double-double ſtar ε Lyræ. Theſe ſhould be kept in view for a conſiderable time, that the eye may acquire the habit of ſeeing ſuch objects well and diſtinctly. The obſerver may next proceed to ξ Urſæ majoris, and the beautiful treble ſtar in Monoceros's right fore-foot; after theſe to *i* Bootis, which is a fine miniature of α Geminorum, to the ſtar preceding α Orionis, and to *n* Orionis. By this time both the eye and the teleſcope will be prepared for a ſtill finer picture, which is η Coronæ borealis. It will be in vain to attempt this latter if all the former, at leaſt *i* Bootis, cannot be diſtinctly perceived to be fairly ſeparated becauſe it is almoſt as fine a miniature of *i* Bootis as that is of *α* Geminorum. If the obſerver has been ſucceſsful in all theſe, he may then, at the ſame time, try *b* Draconis, though I queſtion whether any power leſs than 4 or 500 will ſhew it to be double; but the former I have all ſeen very well with 227.

To try the ſtars of unequal magnitudes it will be expedient to take them in ſome ſuch order as the following: *α* Herculis, *ω* Aurigæ, *δ* Geminorum, *k* Cygni, *ε* Perſei, and *b* Draconis; from theſe the obſerver may proceed to a moſt beautiful object, *ε* Bootis, which I have cloſely attended theſe two years as very proper for the inveſtigation of the parallax of the fixed ſtars.

It appears, from what has been ſaid, that theſe double ſtars are a moſt excellent way of trying a teleſcope; and as the foregoing remarks have ſuggeſted the method of ſeeing how far the power and diſtinctneſs of our inſtruments will reach, I ſhall add the way of finding how much light we have. The obſerver may begin with the pole-ſtar and *α* Lyræ; then go to the ſtar ſouth of *ε* Aquilæ, the treble ſtar near *k* Aquilæ, and laſt of all to the ſtar following *o* Aquilæ. Now, if his teleſcope has not a great deal of good diſtinct light, he will not be able to ſee ſome of the ſmall ſtars that accompany them.

In the ſecond claſs of double ſtars I have put all thoſe that are proper for eſtimations by the eye or very delicate meaſures of the micrometer. To compare the diſtances with the apparent diameters the power of the teleſcope ſhould not be much leſs than 200, as they will otherwiſe be too cloſe for the purpoſe. The inſtrument ought, moreover, to be as much as poſſible free from rays that ſurround a ſtar in common teleſcopes, and ſhould give the apparent diameters of a double ſtar perfectly round and well-defined, with a deep black diviſion between them, as in fig. 6. which repreſents α Geminorum as I have often ſeen it with a power of 460. It will be neceſſary here to take notice, that the eſtimations made with one teleſcope cannot be applied to thoſe made with another: nor can the eſtimations made with different powers, though with the ſame teleſcope, be applied to each other. Whatever may be the cauſe of the apparent diameters of the ſtars, they are certainly not of equal magnitude with the ſame powers in different teleſcopes, nor of proportional magnitude with different powers in the ſame teleſcope. In my inſtruments I have ever found leſs diameter in proportion the higher I was able to go in power, and never have I found ſo ſmall a proportional diameter as when I magnified 6450 times*; therefore if we would wiſh to compare any ſuch obſervations together, with a view to ſee whether a change in the diſtance has taken place, it ſhould be done with the very ſame teleſcope and power, even with the very ſame eye-glaſs or glaſſes; for others, though of equal power and goodneſs, would moſt probably give different proportional diameters of the ſtars.

In the third claſs I have placed all thoſe double ſtars that are more than five but leſs than 15″ aſunder; and for that reaſon, if they ſhould be uſed for obſervations on the parallax of the fixed ſtars, they ought not to be looked upon as quite free from the effects of refraction, &c. In the ſame manner that the ſtars in the firſt and ſecond claſſes will ſerve to try the goodneſs of the moſt capital inſtruments, theſe will afford objects for teleſcopes of inferior power, ſuch as magnify from 40 to 100 times. The obſerver may take them in this or the like order: ζ Urſæ majoris, γ Delphini, γ Arietis, π Bootis, γ Virginis, ι Caſſiopeæ, μ Cygni. And if he can ſee all theſe, he may paſs over into the ſecond claſs, and direct his inſtrument to ſome of thoſe that were pointed out as objects for the very beſt teleſcopes, where, I ſuppoſe, he will ſoon find the want of ſuperior power.

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* See the meaſures of the diameter of α Lyræ. Catalogue of double ſtars, 5th claſs.

The fourth, fifth, and ſixth claſſes contain double ſtars that are from 15 to 30″, from 30″ to 1′, and from 1′ to 2′ or more aſunder. Though theſe will hardly be of any ſervice for the purpoſe of parallax, I thought it not amiſs to give an account of ſuch as I have obſerved; they may, perhaps, anſwer another very important end, which alſo requires a great deal of accuracy, though not quite ſo much as the inveſtigation of the parallax of the fixed ſtars. I will juſt mention it, though foreign to my preſent purpoſe. Several ſtars of the firſt magnitude have already been obſerved, and others ſuſpected, to have a proper motion of their own: hence we may ſurmiſe, that our ſun, with all its planets and comets, may alſo have a motion towards ſome particular part of the heavens, on account of a greater quantity of matter collected in a number of ſtars and their ſurrounding planets there ſituated, which may perhaps occaſion a gravitation of our whole ſolar ſyſtem towards it. If this ſurmiſe ſhould have any foundation, it will ſhew itſelf in a ſeries of ſome years; as from that motion will ariſe another kind of hitherto unknown parallax *, the inveſtigation of which may account for ſome part of the motions already obſerved in ſome of the principal ſtars; and for the purpoſe of determining the direction and quantity of ſuch a motion, accurate obſervations of the diſtance of ſtars that are near enough to be meaſured with a micrometer, and a very high power of

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* See the note in the rev. Mr. MITCHELL's paper on the Parallax of the Fixed Stars, Phil. Tranſ. vol. LVII. p. 252.

teleſcopes may be of conſiderable uſe, as they will undoubtedly give us the relative places of thoſe ſtars to a much greater degree of accuracy than they can be had by tranſit inſtruments or ſectors, and thereby much ſooner enable us to diſcover any apparent change in their ſituation occaſioned by this new kind of ſyſtematical parallax, if I may be allowed to uſe that expreſſion, for ſignifying the change ariſing from the motion of the whole ſolar ſyſtem.

I ſhall now endeavour to deliver a theory of the annual parallax of double ſtars, with the method of computing from thence what is generally called the parallax of the fixed ſtars, or of ſingle ſtars of the firſt magnitude, ſuch as are neareſt to us. It may be obſerved, that the principles upon which I have founded the following theory are of ſuch a nature, that they cannot be ſtrictly demonſtrated, in conſequence of which they are only propoſed as poſtulata, which have ſo great a probability in their favour, that they will hardly be objected to by thoſe who are in the leaſt acquainted with the doctrine of chances.

GENERAL POSTULATA.

1. Let the ſtars be ſuppoſed, one with another, to be about the ſize of the ſun*.

2. Let the difference of their apparent magnitudes be owing to their different diſtances, ſo that a ſtar of the ſecond, third,

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* See Mr. MICHELL's Inquiry into the probable Parallax and Magnitude of the Fixed Stars, Phil. Tranſ. vol. LVII. p. 234. 236. 237. 240. and Dr. HALLEY on the Number, Order, and Light, of the Fixed Stars, Phil. Tranſ. vol. XXXI.

or fourth magnitude is two, three, or four times as far off as one of the firſt *.

In fig. 7. let OE be the whole diameter of the earth's annual orbit; and let *a*, *b*, *c*, be three ſtars ſituated in the ecliptic, in ſuch a manner that they may be ſeen all in one line O*abc*, when the earth is at O. Let the line O*abc* be perpendicular to OE, and draw PE parallel to *c*O. Then, if O*a*, *ab*, *bc*, are equal to each other, *a* will be a ſtar of the firſt magnitude, *b* of the ſecond, and *c* of the third. Let us now ſuppoſe the angle O*a*E, or parallax of the whole orbit of the earth, to be $1''$ of a degree: then we have PE*a* = O*a*E = $1''$.: and, becauſe very ſmall angles, having the ſame ſubtenſe OE, may be taken to be in the inverſe ratio of the lines O*a*, O*b*, O*c*, &c. we ſhall have O*b*E = $\frac{1}{2}''$, O*c*E = $\frac{1}{3}''$, &c. †. Now, when the earth is removed to E, we ſhall have $PEb = EbO = \frac{1}{2}''$, and $PEa - PEb = aEb = \frac{1}{2}''$; that is, the ſtars a, b, will appear to be $\frac{1}{2}''$ diſtant. We alſo have $PEc = EcO = \frac{1}{3}''$, and $PEa - PEc = aEc = \frac{2}{3}''$; that is, the ſtars a, c, will appear to be $\frac{2}{3}''$ diſtant, when the earth is at E. Now, ſince we have $bEP = \frac{1}{2}''$, and $cEP = \frac{1}{3}''$, therefore $bEP - cEP = bEc = \frac{1}{2}'' - \frac{1}{3}'' = \frac{1}{6}''$; that is, the ſtars b, c, will appear to be only $\frac{1}{6}''$ removed from each other, when the earth is at E.

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* The apparent magnitude is here taken in a ſtricter ſenſe than is generally uſed; and by it is rather meant the order into which the ſtars *ought to be* diſtinguiſhed than that into which they *are* commonly divided: for as the order of the magnitudes is here to denote the different relative diſtances, we are to examine carefully the degree of light each ſtar is accurately found to have: and conſidering then that light diminiſhes in the inverſe ratio of the ſquares of the diſtances, we ought to claſs the ſtars accordingly. An allowance ought alſo perhaps to be made for ſome loſs that may happen to the light of very remote ſtars in its paſſage through immenſe tracts of ſpace, moſt probably not quite deſtitute of ſome very ſubtle medium. This conjecture is ſuggeſted to us by the colour of the very ſmall teleſcopic ſtars, for I have generally found them red, or inclining to red; which ſeems to indicate, that the more feeble and refrangible rays of the other colours are either ſtopped by the way, or at leaſt diverted from their courſe by accidental deflections.

† This proves what I have before remarked on the parallax of γ Draconis; for that ſtar, (admitting it to be a ſtar of between the ſecond and third magnitude, which ought to be aſcertained by experiments, as mentioned in the note above), by the poſtulata, will have its place aſſigned ſomewhere between *b*. and *c*, and therefore its parallax will be between $\frac{1}{2}$ and $\frac{1}{3}$ of the parallax of a ſtar of the firſt magnitude. And if Dr. BRADLEY thought that he ſhould have perceived a

From what has been ſaid, we may gather the following general expreſſion, to denote the parallax that will become viſible in the change of diſtance between the two ſtars, by the removal of the earth from one extreme of its orbit to the other. Let P expreſs the total parallax of a fixed ſtar of the firſt magnitude, M the magnitude of the largeſt of the two ſtars, m the magnitude of the ſmalleſt*, and p the partial parallax to be obſerved by the change in the diſtance of a double ſtar; then will $p = \frac{m-M}{Mm}P$; and p being found by obſervation will give us $P = \frac{pMm}{m-M}$. An example or two will explain this ſufficiently. Suppoſe a ſtar of the firſt magnitude ſhould have a ſmall ſtar of the twelfth magnitude near it; then will the partial parallax

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parallax in γ Draconis if at moſt it had amounted to 2″, it follows, that the angle OaE may nearly amount to 4 or 5″ for any thing we can conclude to the contrary from thoſe obſervations.

* As M and m are here taken to expreſs the relative diſtances of the ſtars, in meaſures whereof the diſtance of the neareſt ſtar is taken as unity, thoſe who think the poſtulata on which theſe eſtimations are built cannot be granted, may ſtill uſe the following formulæ, if inſtead of the magnitudes M, m, they put their own eſtimations of the relative diſtances of the ſtars, according to any other method whatever they may think it moſt eligible to adopt; for the apparent magnitude of ſtars is here only propoſed as the moſt probable means we have of forming any conjectures about their relative diſtances.

we are to expect to ſee be $\frac{12\times1}{12-1}$ P; or $\frac{1}{11}$ths of the total parallax of a fixed ſtar of the firſt magnitude; and if we ſhould, by obſervation, find the partial parallax between two ſuch ſtars to amount to 1″, we ſhall have the total parallax P $=\frac{1\times1\times12}{12-1}=1'',0909$. If the ſtars are of the third and twenty-fourth magnitude, the partial parallax will be $\frac{24-3}{3\times24}=\frac{21}{72}$P; and if, by obſervation, *p* is found to be a tenth of a ſecond, the whole parallax will come out $\frac{,1\times3\times24}{24-3}=0'',3428$.

It will be neceſſary to examine ſome different ſituations. Suppoſe the ſtars, being ſtill in the ecliptic, to appear in one line, when the earth is in any other part of its orbit between O and E; then will the parallax ſtill be expreſſed by the ſame algebraic form, and one of the maxima will ſtill lie at O, the other at E; but the whole effect will be divided into two parts, which will be in proportion to each other as radius − ſine to radius + ſine of the ſtars diſtance from the neareſt conjunction or oppoſition.

When the ſtars are any where out of the ecliptic ſituated ſo as to appear in one line O*abc* at rectangles to OE, the maximum of parallax will ſtill be expreſſed by $\frac{m-M}{Mm}$ P; but there will ariſe another additional parallax in the conjunction and oppoſition, which will be to that which is found 90° before or after the ſun, as the ſine (S) of the latitude of the ſtars ſeen at O is to radius (R); and the effect of this parallax will be divided into two parts; half of it lying on one ſide of the large ſtar, the other half on the other ſide of it. This latter parallax, moreover, will be compounded with the former, ſo that the diſtance of the ſtars in the conjunction and oppoſition will then be repreſented by the diagonal of a parallelogram, whereof the two ſemi-parallaxes are the ſides; a general expreſſion for which will be $\sqrt{\overline{\frac{m-M}{2Mm}P}^{2} \times \frac{SS}{RR} + 1}$: for the ſtars will apparently deſcribe two ellipſes in the heavens, whoſe tranſverſe axes will be to each other in the ratio of M to *m* (fig. 8.), and A*a*, B*b*, C*c*, D*d*, will be cotemporary ſituations. Now, if *b*Q be drawn parallel to AC, and the parallelogram *bq*BQ compleated, we ſhall have $bQ = \frac{1}{2}CA - \frac{1}{2}ca = \frac{1}{2}Cc = \frac{1}{2}p$, or ſemi-parallax 90° before or after the ſun, and B*b* may be reſolved into, or is compounded of, *b*Q and *bq*; but $bq = \frac{1}{2}BD - \frac{1}{2}bd =$ the ſemi-parallax in the conjunction or oppoſition. We alſo have $R : S :: bQ : bq = \frac{pS}{2R}$; therefore the diſtance B*b* (or D*d*) $= \sqrt{\overline{\frac{p}{2}}^{2} + \overline{\frac{pS}{2R}}^{2}}$; and by ſubſtituting the value of *p* into this expreſſion we obtain $\sqrt{\overline{\frac{m-M}{2Mm}P}^{2} \times \frac{SS}{RR} + 1}$, as above. When the ſtars are in the pole of the ecliptic, *bq* will become equal to *b*Q, and B*b* will be $.7071\,P\,\frac{m-M}{Mm}$.

Hitherto we have ſuppoſed the ſtars to be all in one line O*abc*; let them now be at ſome diſtance, ſuppoſe 5″ from each other, and let them firſt be both in the ecliptic. This caſe is reſolvable into the firſt; for imagine the ſtar *a*, fig. 9. to ſtand at *x*, and in that ſituation the ſtars *x*, *b*, *c*, will be in one line, and their parallax expreſſed by $\frac{m-M}{Mm}P$. But the angle *a*E*x* may be taken to be equal to *a*O*x*; and as the foregoing form gives us the angles *x*E*b*, *x*E*c*, we are to add *a*E*x*, or 5″ to *x*E*b*, and we ſhall have *a*E*b*. In general, let the diſtance

tance of the ſtars be d, and let the obſerved diſtance at E be D; then will $D=d+p$, and therefore the whole parallax of the annual orbit will be expreſſed by $\frac{DMm-dMm}{m-M}=P$.

Suppoſe the two ſtars now to differ only in latitude, one being in the ecliptic, the other, for inſtance, 5″ north, when ſeen at O. This caſe may alſo be reſolved by the former; for imagine the ſtars b, c, fig. 7. to be elevated at rectangles above the plane of the figure, ſo that aOb, or aOc, may make an angle of 5″ at O: then, inſtead of the lines $Oabc$, Ea, Eb, Ec, EP, imagine them all to be planes at rectangles to the figure; and it will appear, that the parallax of the ſtars in longitude muſt be the ſame as if the ſmall ſtar had been without latitude. And ſince the ſtars b, c, by the motion of the earth from O to E, will not change their latitude, we ſhall have the following conſtruction for finding the diſtance of the ſtars, ab, ac, at E, and from thence the parallax P. Let the triangle $ab\beta$, fig. 10. repreſent the ſituation of the ſtars; ab is the ſubtenſe of 5″, that being the angle under which they are ſuppoſed to be ſeen at O. The quantity $b\beta$ by the former theorem is found $\frac{m-M}{Mm}P$, which is the partial parallax that would have been ſeen by the earth's moving from O to E, had both ſtars been in the ecliptic; but on account of the difference in latitude it will now be repreſented by $a\beta$, the hypothenuſe of the triangle $ab\beta$: therefore, in general, putting $ab=d$, and $a\beta=D$, we have $\frac{\sqrt{DD-dd}\times Mm}{m-M}=P$. Hence D being taken by obſervation and d, M, and m, given, we obtain the total parallax.

If the ſituation of the ſtars differs in longitude as well as latitude, we may reſolve this caſe by the following method.

Let the triangle $ab\beta$, fig. 11. repreſent the ſituation of the ſtars, $ab=d$ being their diſtance ſeen at O, $a\beta=D$ their diſtance ſeen at E. That the change $b\beta$ which is produced by the earth's motion will be truly expreſſed by $\frac{m-M}{Mm}P$, may be proved as before, by ſuppoſing the ſtar a to have been placed at α. Now let the angle of poſition $ba\alpha$ be taken by a micrometer*, or by any other method that may be thought ſufficiently exact; then, by ſolving the triangle $ab\alpha$, we ſhall have the longitudinal and latitudinal differences $a\alpha$ and $b\alpha$ of the two ſtars. Put $a\alpha=x$, $b\alpha=y$, and it will be $x+b\beta=aq$, whence $D=\sqrt{\overline{x+\frac{m-M}{Mm}P}|^2+yy}$; and $\frac{\sqrt{D^2-y^2}\times M^2m^2-xMm}{m-M}=P$.

If neither of the ſtars ſhould be in the ecliptic, nor have the ſame longitude or latitude, the laſt theorem will ſtill ſerve to calculate the total parallax whoſe maximum will lie in E. There will, moreover, ariſe another parallax, whoſe maximum will be in the conjunction and oppoſition, which will be divided, and lie on different ſides of the large ſtar; but as we know the whole parallax to be exceedingly ſmall, it will not be neceſſary to inveſtigate every particular caſe of this kind; for, by reaſon of the diviſion of the parallax, which renders obſervations taken at any other time, except where it is greateſt, very unfavourable, the forms would be of little uſe.

To finiſh this theory, I ſhall only add a general obſervation on the time and place where the maxima of parallax will happen.

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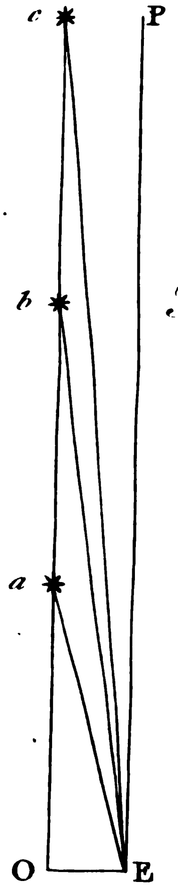
* The poſition of a line paſſing through the two ſtars, with the parallel of declination of the largeſt of them, may be had by the micrometer I invented for this purpoſe in the year 1779, of whieh a deſcription has been given in a former paper; whence, by ſpherical trigonometry, we eaſily deduce their poſition $ba\alpha$ fig. 11. with regard to the ecliptic.

When



Fig

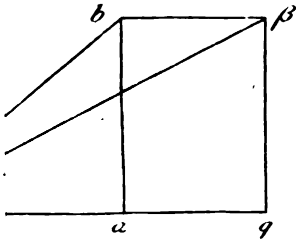
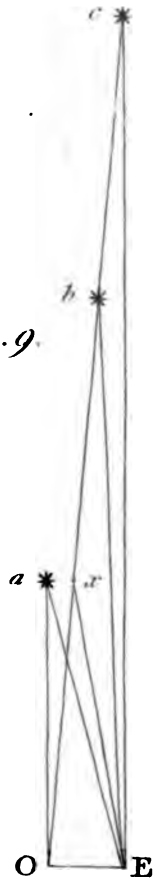
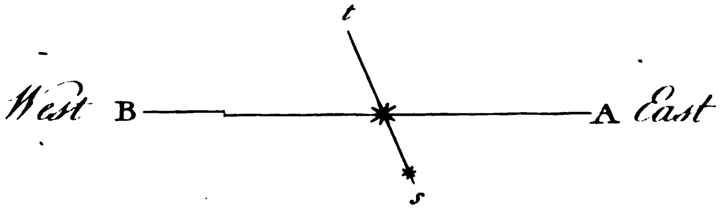


Fig. 9.





When two unequal ſtars are both in the ecliptic, or, not being in the ecliptic, have equal latitudes, north or ſouth, and the largeſt ſtar has moſt longitude, the maximum of the apparent diſtance will be when the ſun's longitude is 90° more than the ſtars, or when obſerved in the morning; and the minimum when the longitude of the ſun is 90° leſs than that of the ſtar, or when obſerved in the evening.

When the ſmall ſtar has moſt longitude, the maximum and minimum, as well as the time of obſervation, will be the reverſe of the former.

When the ſtars differ in latitudes, this makes no alteration in the place of the maximum or minimum, nor in the time of obſervation; that is to ſay, it is immaterial whether the largeſt ſtar has the leaſt or the moſt latitude of the two ſtars.

XII. *Catalogue of Double Stars. By Mr.* Herſchel, *F. R. S. communicated by Dr.* Watſon, *Jun.*

Read January 10, 1782.

INTRODUCTORY REMARKS.

THE following catalogue contains not only double-ſtars, but alſo thoſe that are treble, double-double, quadruple, double-treble, and multiple. The particulars I have given of them are comprehended under the following general heads:

I. The names of the ſtars and number in FLAMSTEAD's Catalogue; or, if not contained therein, ſuch a deſcription of their ſituation as will be found ſufficient to point them out.

II. The comparative ſize of the ſtars. On this occaſion I have uſed the terms equal, a little unequal, pretty unequal, conſiderably unequal, very unequal, extremely unequal, and exceſſively unequal, as expreſſing the different gradations to which I have endeavoured to affix always the ſame meaning.

III. The colours of the ſtars as they appeared to me when I viewed them. Here I muſt remark, that different eyes may perhaps differ a little in their eſtimations. I have, for inſtance, found, that the little ſtar which is near α Herculis, by ſome to whom I have ſhewn it has been called green, and by others blue. Nor will this appear extraordinary when we recollect that there are blues and greens which are very often, particularly by candle-light, miſtaken for each other. The ſituation will alſo affect the colour a little, making a white ſtar appear pale red when the altitude is not ſufficient to clear it of the vapours. It is difficult to find a criterion of the colours of ſtars, though I might in general obſerve that Aldebaran appears red, Lyra white, and ſo on; but when I call the ſtars garnet, red, pale red, pale roſe-colour, white inclining to red, white, white inclining to blue, blueiſh white, blue, greeniſh, green, duſky, I wiſh rather to refer to the double ſtars themſelves to explain what is meant by thoſe terms.

IV. The diſtances of the ſtars are given ſeveral different ways. Thoſe that are eſtimated by the diameter can hardly be liable to an error of ſo much as one quarter of a ſecond; but here muſt be remembered what I have before remarked on the comparative appearance of the diameters of ſtars in different inſtruments. Thoſe that are meaſured by the micrometer, I fear, may be liable to an error of almoſt a whole ſecond; and if not meaſured with the utmoſt care, to near 2″. This is, however, to be underſtood only of ſingle meaſures; for the diſtance of many of them that have been meaſured very often in the courſe of two years obſervations can hardly differ ſo much as half a ſecond from truth, when a proper mean of all the meaſures is taken. As I always make the wires of my micrometer outward tangents to the apparent diameter of the ſtars, all the meaſures muſt be underſtood to include both their diameters; ſo that we are to deduct the two ſemi-diameters of the ſtars if we would have the diſtance of their centers. What I have ſaid concerns only the wire micrometers, for my laſt new micrometer is of a ſuch a conſtruction, that it immediately gives the diſtance of the centers and its meaſures (as far as in a few months Ihave been able to find out) may be relied on to about one-tenth of a ſecond, when a mean of three obſervations is taken. When I have added *inaccurate*, we may ſuſpect an error of 3 or 4″. *Exactly eſtimated* may be taken to be true to about one-eighth part of the whole diſtance; but only *eſtimated*, or *about*, &c. is in ſome reſpect quite undetermined; for it is hardly to be conceived how little we are able to judge of diſtances when, by conſtantly changing the powers of the inſtrument, we are as it were left without any guide at all. I ſhould not forget to add, that the meaſure of ſtars, whereof one is extremely ſmall, muſt claim a greater indulgence than the reſt on account of the difficulty of ſeeing the wires when the field of view cannot be ſufficiently enlightened.

V. The angle of poſition of the ſtars I have only given with regard to the parallel of declination, to be reduced to that with the ecliptic as occaſion may require. The meaſures always ſuppoſe the large ſtar to be the ſtandard, and the ſituation of the ſmall one is deſcribed accordingly. Thus in figure 12. AB repreſents the apparent diurnal motion of a ſtar in the direction of the parellel of declination AB; and the ſmall ſtar is ſaid to be ſouth preceding at *mn*, north preceding at *op*, ſouth following at *qr*, and north following at *st*. The meaſure of theſe angles, I believe, may be relied upon to 2° or at moſt 3°, except when mentioned inaccurate, where an error amounting to 5° may poſſibly take place. In mere eſtimations of the angle, without any wires at all, an error may amount to at leaſt 10°, when the ſtars are near each other.

VI. The dates when I firſt perceived the ſtars to be double, treble, &c. are marked in the margin of each ſtar.

To ſhorten the work as much as poſſible, I have put L. for the large ſtar; S. for the ſmall ſtar; w. for white; r. for red; d. for duſky; n. for north; ſ. for ſouth; and have likewiſe occaſionally uſed other abbreviations that will be eaſily underſtood.

It may be ſeen, that this catalogue is yet in a very imperfect ſtate, many of the ſtars not having even the principal elements of diſtance and poſition determined with any degree of accuracy; but having already mentioned the reaſon why I give it imperfect as it is, I can only add that my endeavours will not be wanting ſoon to remove thoſe defects. However, ſince this can only be a work of ſome time, we may hope, in the mean while, that many lovers of the ſcience will turn their thoughts upon the ſame ſubject.

CATALOGUE OF DOUBLE STARS.

FIRST CLASS.

1. ε Bootis. FLAMST. 36. Ad dextrum femur in perizomate.

Sept. 9. 1779. Double. Very unequal. L. reddiſh; S. blue, or rather a faint lilac. A very beautiful object. The vacancy, or black diviſion between them, with 227 is ¾ diameter of S.; with 460, 1¼ diameter of L.; with 932, near 2 diameters of L.; with 1159, ſtill farther; with 2010 (extremely diſtinct) 2¼ diameters of L. Theſe quantities are a mean of two years obſervation. Poſition 31° 34′ n. preceding.

2. ξ Urſæ majoris. FL. 53. In dextro poſteriore pede.

May 2, 1780. Double. A little unequal. Both w. and very bright. The interval with 222 is ⅔ diameter of L.; with 227, 1 diameter of L.; with 278, near 1½ diameter of L. Poſition 53° 47′ ſ. following.

3. σ Coronæ borealis, FL. 17.

Aug. 7, 1780. Treble. The two neareſt pretty unequal; the third very faint with powers lower than 460. The two neareſt both w.; the third d. Interval of the two neareſt with 227, full 1¼ diameter of L.; with 460, 2 diameters of L. Poſition 77° 32′ n. preceding. Diſtance of the third from L. 24″ by exact eſtimation. Poſition 25° n. following by eſtimation.

4. In conſtellatione Draconis, FL. 16.

Aug. 8, 1780. Double. It is the ſtar to which a line drawn from ν through μ points, at nearly the ſame diſtance from μ as μ from ν. Conſiderably unequal. L. w.; S. w. inclining to r. With 222, 1 diameter of L.; with 278, 1½ diameter of L. Poſition 24° 0′ ſ. following. There is a third ſtar, at ſome diſtance, preceding.

5. σ Caſſiopeæ, FL. 8. In dextro cubito.

Aug. 31, 1780. Double. It is the ſtar at the vertex of a teleſcopic iſoſceles triangle turned to the ſouth. Very unequal. L. w. a little inclining to r.; S. d. With 222, near 1 diameter of L.; with 460, 1½ diameter of L. Poſition 60° 28′ n. preceding.

6. Quæ infra oculum Lyncis, FL. 12.

Oct. 3, 1780. A curious treble ſtar. Two neareſt pretty unequal. L. w.; S. w. inclining to roſe colour. With 227, about ½ diameter; with 460, full ¾ diameter of S. Poſition 88° 37′ ſ. preceding. The firſt and third conſiderably unequal; ſecond and third pretty unequal. The third pale r. Diſtance from the firſt 9″ 23‴; too difficult to be extremely exact. Poſition with regard to the firſt 32° 33′ n. preceding.

7. δ Draconis,

7. *b* Draconis, FL. 39. Trium in recta, in prima inflectione colli, borea.

Oct. 3. 1780. A minute double ſtar. Extremely unequal, the ſmall ſtar being a fine lucid point. L. w.; S. inclining to r. With 227, ½ diameter of L.; with 460, full 1½ diameter of L.; with 932 (extremely fine) full 2 diameters of L. Poſition 77° 8′ n. following. A third ſtar at ſome diſtance; duſky r. Poſition 63° 22′ n. following.

8. ε Draconis, FL. 63. In quadrilatero inflexionis primæ.

Oct. 3, 1780. A very minute double ſtar. Exceſſively unequal; the ſmall ſtar can only be ſeen when the air is perfectly clear. L. w.; S. d. With 227, leſs than 1 diameter of L.; with 278, not a diameter of L. Poſition 63° 14′ n. preceding. A pretty large third ſtar at about 3 or 4′. Poſition of this third ſtar with ε 88° 16′ n. following.

9. In cauda Lyncis media, FL. 38.

Nov. 24. 1780. Double. Very unequal. L. w.; S. inclining to r. With 227, extremely cloſe; with 460, at leaſt ½ diameter of S. A very fine object. Poſition 25° 51′ ſ. preceding. A proper motion is ſuſpected in one of the ſtars.

10. In ſiniſtro anteriore pede Monocerotis, FL. 11.

Feb. 15. 1781. A curious treble ſtar; may appear double at firſt ſight; but with ſome attention we ſee that one of them again is double. The firſt, or ſingle ſtar, is the largeſt; the other two are both ſmaller, and almoſt equal, but the preceding of them is rather larger than the following. They are all w. The two neareſt with 227, 1 diameter of the preceding, or nearly 1¼ of the following; with 460, 1¼ diameter of the preceding. Poſition of the two neareſt 11° 32′ ſ. following. For an account of the ſingle ſtar, ſee the ſecond claſs. As perfect as I have ſeen this treble ſtar with 460, it is one of the moſt beautiful ſights in the heavens; but requires a very fine evening.

11. In conſtellatione Cancri, FL. 11.

Mar. 13, 1781. Double. Conſiderably unequal. Both pale r. With 227, 1 full diameter of L.; with 460, about 1¾ diameter of L. Poſition 85° 10′ n. preceding.

12. *d* Serpentis, FL. 59. In Cauda.

July 17, 1781. Double. Very unequal. L. reddiſh w.; S. fine blue. With 227, 1 full diameter of L.; with 278, 1⅓ diameter of L. Poſition 44° 33′ n. preceding.

13. In conſtellatione Aquilæ, near FL. 37.

July 25, 1781. A curious treble ſtar. It is the laſt ſtar of a teleſcopic trifolium n. following *k*, ſimilar to that in the hand of Aquarius. The two neareſt very unequal; the third ſtar exceſſively ſmall, and not viſible with 227. The two neareſt with 460, no more than ½ diameter of L.; the fartheſt about 7 or 8″.

14. In conſtellatione Aquilæ, FL. 24.

July 30, 1781. Double. In HARRIS's maps it is the ſtar in the elbow of Antinous. Exceſſively unequal; the ſmall ſtar is but juſt viſible with 227; but with 460 it is pretty ſtrong. L. pale r.; S. d. With 227, 1 full diameter of L.; with 460, 1½ diameter of L. Poſition 72° 0′ ſ. following.

15. *i* Bootis, FL. 44.

Aug. 17, 1781. Double. In HARRIS's maps it is marked *i*, but has no letter in FL. Atlas. Conſiderably unequal. Both

w.

w. With 227 they ſeem almoſt to touch, or at moſt ¼ diameter of S. aſunder; with 460, ½ or ¼ diameter of S. This is a fine object to try a teleſcope, and a miniature of α Geminorum. Poſition 29° 54′ n. following.

16. η Coronæ borealis, FL. 2.

Sept. 9. 1781. Double. A little unequal. They are whitiſh ſtars. They ſeem in contact with 227, and though I can ſee them with this power, I ſhould certainly not have diſcovered them with it; with 460, leſs than ¼ diameter; with 932, fairly ſeparated, and the interval a little larger than with 460. I ſaw them alſo with 2010, but they are ſo cloſe that this power is too much for them, at leaſt when the altitude of the ſtars is not very conſiderable; with 460 they are as fine a miniature of ι Bootis as that is of α Geminorum. Poſition 59° 19′ n. following.

17. In conſtellatione Bootis, near FL. 51.

Sept. 10. 1781. Double. It is a ſtar near μ not marked in FLAMSTEAD's Catalogue. Conſiderably unequal. Both duſky w. inclined to r. The interval with 460 is ¼ diameter of S. The poſition of the ſmall ſtar is turned towards μ a little following the line which joins L to μ Bootis. See μ Bootis in the ſixth claſs.

18. In conſtellatione Coronæ borealis.

Sept. 10. 1781. Double. It is the ſmalleſt of two teleſcopic ſtars between θ and δ, not contained in FL. Cat. Equal. Both d. With 460, about 1¼ diameters. Poſition 21° 0′ n. following.

19. *b* Draconis, near FL. 19.

Sept. 10. 1781. One of the moſt minute of all the double ſtars I have hitherto found. It is the ſmall teleſcopic ſtar near the preceding *b* Draconis. Conſiderably unequal. Both duſky w. inclining to r. With 460, they ſeem in contact; I have however had a very good view of a ſmall dark diviſion between them. Poſition (by exact eſtimation) 25 or 30° ſ. preceding. They are too minute for any micrometer I have. It is in vain to look for them if every circumſtance is not favourable. The obſerver as well as the inſtrument muſt have been long enough out in the open air to acquire the ſame temperature. In very cold weather, an hour at leaſt will be required; but in a moderate temperature, half an hour will be ſufficient.

20. In dextro humero Orionis, FL. 52.

Oct. 1. 1781. Double. A little unequal. Both w. a little inclining to pale r. With 227, ¼ diameter; with 460, ½ diameter. Poſition 69° 41′ ſ. preceding.

21. *c* Trianguli, near FL. 12. and 13.

Oct. 8. 1781. Double. It is the moſt north of a ſmall teleſcopic trapezium of unequal ſtars. Extremely unequal. With 460, ¼ diameter of L. Poſition (by eſtimation) 55 or 60° n. preceding.

22. *n* Orionis, FL. 33. Duarum præcedentium 13^{um} (*a*) antecedens.

Oct. 22. 1781. Double. Conſiderably unequal. L. w.; S. w.; inclining to blue. With 227, they ſeem almoſt in contact; with 460, ½ diameter of S. Poſition 60° 55′ n. following. A very pleaſing object and eaſily ſeen.

23. In poſterioribus femoribus Canis minoris.

Nov. 21, 1781. A moſt minute double ſtar. It is the ſmall teleſcopic ſtar following Procyon. A little unequal. Both w. With 278, $\frac{1}{3}$ of a diameter of S.; with 460, near $\frac{1}{4}$ of a diameter of S. They are cloſer than η Coronæ, becauſe their diameters, by which they are eſtimated, are ſmaller. Poſition 27° 21′ ſ. following. To ſee this very minute double ſtar well, Procyon ſhould be near its meridian altitude. There is a ſmall teleſcopic ſtar preceding the double ſtar. Diſtance 1′ 59″ 39‴ from center to center.

24. ζ Cancri, FL. 16.

Nov. 21, 1781. A moſt minute treble ſtar. It will at firſt ſight appear as only a double ſtar, but with proper attention, and under favourable circumſtances, the preceding of them will be found to conſiſt of two ſtars, which are conſiderably unequal. The largeſt of theſe is larger than the ſingle ſtar; and the leaſt of the two is leſs than the ſingle ſtar. The firſt and ſecond (in the order of magnitude) pretty unequal. The ſecond and third pretty unequal. The two neareſt both pale r. or r. With 278, but juſt ſeparated; with 460, $\frac{1}{4}$ diameter of S. Poſition 86° 32′ n. following. For meaſures relating to the third or ſingle ſtar ſee ζ Cancri in the third claſs of double ſtars.

SECOND CLASS OF DOUBLE STARS.

1. † α Geminorum, FL. 66. In capite præcedentis II^i.

April 8, 1778. Double. A little unequal. Both w. The vacancy between the two ſtars, with a power of 146, is 1 diameter of S.; with 222, a little more than 1 diameter of L.; with 227, 1½ diameter of S.; with 460, near 2 diameters of L.; (ſee fig. 6.) with 754, 2 diameters of L.; with 932, full 2 diameters of L.; with 1536 (very fine and diſtinct) 3 diameters of L.; with 3168, the interval extremely large, and ſtill pretty diſtinct. Diſtance by the micrometer 5″,156. Poſition 32° 47′ n. preceding. Theſe are all a mean of the laſt two years obſervations, except the firſt with 146.

2. † α Herculis, FL. 64. In capite.

Aug. 29, 1779. A beautiful double ſtar. Very unequal. L. r.; S. blue inclining to green; the colours with every power the ſame. The interval with 222, 1¼ diameter of L.; with 227, above 2 diameters of L.; with 932, above 3 diameters of L. Diſtance 4″966. All a mean of two years obſervations. A ſingle meaſure with my laſt new micrometer, from center to center, 4″ 34‴. Poſition 30° 35′ ſ. following.

3. * ρ Herculis, FL. 75. Trium in ſiniſtro femore, tertia.

Aug. 29, 1779. Double. Pretty unequal. Both w. With 227, 1¼ diameter of L.; with 460, 2 diameters of S. Diſtance 2″,969. Poſition 30° 21′ n. preceding. The meaſures a mean of two years obſervations.

3 4.

4. * *p* Serpentarii, FL. 70. Tres has ſequitur, quaſi ſupra mediam.

Aug. 29. 1779. Double. Conſiderably unequal. L. w.; S. inclining to r. With 227, $1\frac{4}{5}$ diameter of L.; with 460, much above 2 diameters of L. Poſition 9° 14′ ſ. following. Mean of two years obſervations.

5. et 6. * ε Lyra, FL. 4. and 5.

Aug. 29. 1779. A very curious double-double ſtar. At firſt ſight it appears double at ſome conſiderable diſtance, and by attending a little we ſee that each of the ſtars is a very delicate double ſtar. The firſt ſet conſiſts of ſtars that are conſiderably unequal. The ſtars of the ſecond ſet are equal, or the preceding of them rather larger than the following. The colour of the ſtars in the firſt ſet L. very w.; S. a little inclining to r. In the ſecond ſet both w. The interval between the ſtars of the unequal ſet, with a power of 227, is full 1 diameter of L.; with 460, near $1\frac{1}{2}$ diameter of L.; with 932, full $1\frac{1}{2}$ diameter; with 2010, $2\frac{1}{5}$ diameters. The interval between the equal ſet with a power of 227 is almoſt $1\frac{1}{4}$ diameter of either; with 460, full $1\frac{1}{4}$ diameter; with 932, 2 diameters; with 2010, $2\frac{1}{4}$ diameters. Theſe eſtimations are a mean of two years obſervations, Poſition of the unequal ſet 56° 0′ n. following. Poſition of the equal ſet 72° 57′ ſ. following.

7. * ζ Aquarii, FL. 55. Trium in manu dextra præcedens.

Sept. 12. 1779. Double. Equal, or the preceding rather the largeſt. Both w. With 227, $1\frac{1}{4}$ diameter; with 449, $1\frac{3}{5}$ diameter; with 460, 2 diameters; with 910, near 2 diameters; with 932, $2\frac{1}{4}$ diameters;

with 2010, pretty diſtinct; but too tremulous to eſtimate. With my 20 feet reflector, power 600, full 2 diameters, very diſtinct. Poſition, 71° 39′ n. following. Diſtance 4″,56, mean of two years obſervation.

8. ζ Coronæ borealis, FL. 7.

Oct. 1. 1779. Double. Conſiderably unequal. L. fine w. S. w. inclining to r. With 222, almoſt 3 diameters of L. Diſtance 5″,468. Poſition 25° 51′ n. preceding, mean of two years obſervations.

9. λ Orionis, FL. 39. In capite nebuloſa.

Oct. 7. 1779. Quadruple, or rather a double ſtar and two more at a ſmall diſtance. The double ſtar conſiderably unequal. L. w. S. pale roſe colour. With 222, 1½ diameter of L.; with 449, above two diameters of L. Diſtance 5″,833, a mean of all the meaſures. Poſition 45° 14′ n. following. As every one of the four ſtars is perfectly diſtinct, it is evident, the whole appeared nebulous to FLAMSTEAD for no other reaſon than becauſe his teleſcope had not ſufficient power to diſtinguiſh them.

10. and 11. σ Orionis, FL. 48. Ultimam cinguli præcedit ad auſtrum.

Oct. 7. 1779. A double-treble ſtar, or two ſets of treble ſtars, almoſt ſimilarly ſituated. Preceding ſet. The two neareſt equal; the third larger and, compared with either of the former two, pretty unequal. The two neareſt with 222, about 2 diameters. Poſition of the following ſtar of the two neareſt with the third 66° 35′ ſ. preceding. Poſition of the two neareſt, by exact eſtimation, 2 or 3° n. following or ſ. preceding the following ſet. The two neareſt very unequal. The largeſt of the two and the fartheſt conſiderably unequal. L. w.; S. blueiſh. The two neareſt with 222, about 2½ diameters of L.; the two fartheſt 43″ 12‴. Poſition of the two neareſt 5° 5′ n. following. Poſition of the two fartheſt 29° 4′ n. following. A pretty object with 227.

12. α Piſcium, FL. ultima. In nodo duorum linorum.

Oct. 19, Double. Conſiderably unequal. Both w. With
1779. 222, not quite 2 diameters of L.; with 460, about 3 diameters of L. Diſtance 5″,123 mean meaſure. Poſition 67° 23′ n. preceding.

13. μ Draconis, FL. 21. In lingua.

Oct. 19. Double. Equal. Both w. With 227, 1½ dia-
1779. meter; with 460, 2½ diameters. Diſtance 4″,354 mean meaſure. Poſition 37° 38′ ſ. preceding or n. following.

14. ω Aurigæ, FL. 4.

Oct. 30, Double. Very unequal. L. w.; S. r. With
1779. 227, almoſt 2 diameters of L.; with 460, full 3 diameters of L. Poſition 82° 37′ n. preceding.

15. ψ Cygni, FL. 24. In ala dextra.

Nov. 2, Double. Extremely unequal; the ſmall ſtar a mere
1779. point. L. w.; S. r. With 227, near 1¼ diameter of L; with 278, near 1½ diameter of L.; with 460, 2 diameters of L. Poſition 89° 32′ n. preceding.

16. ξ Cephei, FL. 17. In pectore.

Nov. 7, A fine double ſtar. Conſiderably unequal. L. w.
1779. inclining to r.; S. duſky grey. With 222, nearly 2 diameters of L. Single meaſure 5″,90. Poſition 20° 18′ n. preceding.

17. * In ſiniſtro anteriore pede Monocerotis, FL. 11.

Dec. 5, 1779. Double. With 222, about 1½ diameter. Poſition (taken Oct. 20, 1781) with the fartheſt of the other two ſtars 31° 38′ ſ. following. See the tenth ſtar in the firſt claſs.

18. ξ Bootis, FL. 37.

April 9, 1780. Double. Very unequal. L. pale r. or nearly r. S. garnet, or deeper r. than the other. With 222, 1½ diameter of L, with 460, full 3 diameters of L. Diſtance 3″ 23‴ ſingle meaſure. Poſition 65° 53′ n. following.

19. g Serpentarii, FL. 5.

May 2, 1780. Double. It is a ſtar in the body of Cancer, and the double ſtar is at the angular point of the three teleſcopic g's making a rectangle. Pretty unequal. Both w. With 227, 1½ diameter of L. Poſition 82° 10′ ſ. preceding.

20. and 21. ξ Libræ, FL. ultima.

May 23, 1710. Double double. The firſt ſet very unequal. L. fine w. With 227, nearly 2 diameters of L*. By the micrometer 6″ 23‴, but too large a meaſure. Poſition 1° 23″ n. following. The other ſet both ſmall and obſcure. With 227, perhaps 5 or 6 of their diameters aſunder.

22. ε Perſei, FL. 45. In ſiniſtro genu.

Aug. 2, 1780. Double. Extremely unequal. L. w.; S. d. With 222, 2½ diameters of L. Poſition 81° 28′ ſ. following, a little inaccurate. A third ſtar near at about 1½ or 1¾ min.

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* In a future collection this ſet will be found as a treble ſtar of the firſt claſs, the large white ſtar, with a power of 460 and 932, appearing to be two ſtars.

23. In conſtellatione Serpentarii, near FL. 11.

Aug. 7, 1780. Double. It is the ſmalleſt and preceding of two in the finder. Pretty unequal. L. pale r.; S. duſky r. With 222, about 1¼ diameter of L.; with 278, about 1½ diameter of L.; with 460, above 2 diameters of L. Poſition 46° 24′ n. preceding. A little inaccurate.

24. In conſtellatione Aquarii, FL. 108. In ſequenti flexu 5ª ad A.

Aug. 23, 1780. Double. In HARRIS's maps it is marked *i*. Unequal. With 227, 2 diameters; with 460, about 3 diameters.

25. *k* Cygni, FL. 52.

Sept. 8, 1780. Double. Extremely unequal. L. w. inclining to r.; S. d. and extremely faint; with 227, 2½ diameters of L.; with 460, about 4 diameters of L. or more. Poſition 28° 17′ n. following.

26. In conſtellatione Orionis, near FL. 42. In longo enſis.

Oct. 23, 1780. Double. It is the moſt north of three teleſcopic ſtars in a line at the end of a cluſter near *c*. Extremely unequal. L. w.; S. d. With 278, 1¼ diameter of L. Poſition 26° 5′ n. following.

27. δ Geminorum, FL. 55. In inguine ſiniſtro ſequentis IIⁱ.

Mar. 13, 1781. Double. Extremely unequal. L. w. inclining to r.; S. r. With 227, about 2½ full diameters of L.; with 460, 4 or 5 diameters. Poſition 85° 51′ ſ. preceding.

28. In conſtellatione Aquilæ, near FL. 54.

July 23, 1781. Double. It is a ſtar following *o*. Exceſſively unequal. The ſmall ſtar is not viſible with 227, nor with 278. It is viſible with 460; but not without attention. Diſtance with 460, about 4 or 5 diameters of L. Poſition, by very exact eſtimation, 36° 28′ n. preceding.

29. In conſtellatione Aquilæ, near FL. 63. In medio capite.
July 31, Double. It is the ſtar at the vertex of a teleſcopic
1781. iſoſceles triangle near τ. Extremely unequal. Both
r. With 460, 2 diameters of L. Poſition 75° 48′ n. preceding.

30. ζ Sagittæ, FL. 8. Trium in arundine ſequens.
Aug. 23, Double. Extremely unequal. The ſmall ſtar
1781. brighter with 460 than with 227 or with 278; with
460, between 4 or 5 diameters of L.; with 278, 2½ diameters of L. Diſtance 5″ 27‴ inaccurate. Poſition 34° n. preceding.

31. In conſtellatione Draconis, FL. 56.
Sept. 6, Double. A little unequal. Both w. With 460,
1781. near 9 diameters. Diſtance 5″ 7‴.

32. In conſtellatione Sagittæ, near FL. 4.
Sept. 7, Double. It is the ſtar north following ι. L. pale
1781. r.; S. d. Diſtance 5″ 3‴ inaccurate.

33. β Orionis, FL. 19. In ſiniſtro pede ſplendida.
Oct. 1, Double. Extremely unequal. L. w.; S. inclin-
1781. ing to r. With 227, 2¾ or 2½ diameters of Rigel. With 460, more than 3 diameters of L. Diſtance 6″ 27‴. Poſition 68° 12′ ſ. preceding. The ſmall ſtar not wanting apparent magnitude is better to be ſeen with my power of 227 than with 460.

34. ι Trianguli, FL. 6.
Oct. 8, Double. It is marked *b* in the ſmall triangle of
1781. HARRIS's maps. Very unequal. L. pale r. or reddiſh w.; S. blueiſh r. With 227, full 1½ diameter

of L.; with 460, full 1½ diameter of L. Poſition 4° 23′ n. following. A pretty object, ſomewhat reſembling α Herculis, but ſmaller and not ſo bright.

35. In conſtellatione Trianguli, near FL. 6.

Oct. 8, 1781. Double. It is the ſtar following ι. Equal. Both duſky w. With 460, about 2½ diameters.

36. In conſtellatione Eridani, FL. 32.

Oct. 22. 1781. Double. Conſiderably unequal. L. reddiſh w.; S. blue. Diſtance 4″ 19‴. Poſition 73° 23′ n. preceding.

37. In capite Monocerotis.

Oct. 22. 1781. Double. It is one of a cluſter of ſix teleſcopic ſtars, arranged in pairs.

38. In conſtellatione Bootis.

Dec. 24, 1781. Double. It is the moſt north and largeſt of three in a line, ſ. following FL. 15. Conſiderably unequal, L. w.; S. inclining to r. Diſtance 5″ 10‴. Poſition 83° 5′ ſ. preceding.

THIRD CLASS OF DOUBLE STARS.

1. θ Orionis, FL. 41. Trium contiguarum in longo enſis media.

Nov. 15, 1776. Quadruple. It is the ſmall teleſcopic Trapezium in the Nebula. Conſiderably unequal. The moſt ſouthern ſtar of the following ſide of the Trapezium is the largeſt; the ſtar in the oppoſite corner is the ſmalleſt; the remaining two are nearly equal. L. pale r.; the ſtar preceding L. inclined to garnet; following L. inclined to garnet; oppoſite to L. d. With 460, the ſtars are all full, round, and well-defined.

The two ſtars in the preceding ſide diſtance 8″,780; in the ſouthern ſide, 12″,812; in the following ſide 15″,208; in the northern ſide, 20″,396.

2. ζ Urſæ majoris, FL. 59. Trium in cauda media.

Aug. 17, 1779. Double. Conſiderably unequal. L. w; S. w. inclining to pale roſe colour. Diſtance 14″,5 by two years obſervation, not a mean but that which I ſuppoſe neareſt the truth. Poſition 56° 46′ ſ. following.

3. η Caſſiopeæ, FL. 24. In cingulo.

Aug. 17, 1779. Double. Very unequal. L. fine w.; S. fine garnet, both beautiful colours. Diſtance 11″,275 mean meaſure. Poſition 27° 56′ n. following.

4. In extremitate pedis Caſſiopeæ, FL. 55. ι Ptolemæi.

Aug. 17, 1779. Double. Extremely unequal. L. w.; S. blueiſh r. Diſtance 7″,5 ſingle meaſure. Poſition 10° 37′ ſ. following ‡.

5. * γ Andromedæ, FL. 57. Supra pedem ſiniſtrum.

Aug. 25, 1779. Double. Very unequal. L. reddiſh w.; S. fine light ſky-blue, inclining to green. Diſtance 9″,2541 a mean of two years obſervation. Poſition 19° 37′ n. following. A moſt beautiful object.

6. β Cephei, FL. 8. In cingulo ad dextrum latus.

Aug. 31, 1779. Double. Very unequal. L. blueiſh w.; S. garnet. Diſtance 13″,125. Poſition 15° 28′ ſ. preceding.

7. * β Scorpii, FL. 8. Trium in fronte, lucidarum; borea.

Sept. 19, 1779. Double. Very unequal. L. whitiſh r; S. r. Diſtance 14″,375. Poſition 64° 51′ n. following.

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‡ In a future collection this will be found as a treble ſtar of the firſt claſs; the large ſtar having a ſmall one preceding, eaſily ſeen with 460 and 932.

8. * π Bootis, FL. 29.

Sept.20, 1779. Double. Pretty unequal. L. w.; S. w. inclining to r. Diſtance 6″,171. Poſition 6° 28′ ſ. following.

9. † γ Arietis, FL. 5. Quæ in cornu duarum præcedens.

Sept.27, 1779. Double. Equal, or if any difference the following is the largeſt. Diſtance 10″,172, a mean of two years obſervation. L. w. inclining a little to r.; S. w. Poſition 86° 5′ n. preceding.

10. * γ Delphini, FL. 12. Borea ſequentis lateris, quadrilateri.

Sept.27, 1779. Double. Nearly equal, the following a little larger. Both w. Diſtance 11″,822, being a mean of the meaſures taken in Sept. Oct. Nov. and Dec. 1779. As I ſuſpect a motion in one of theſe ſtars, I thought it beſt not to join other obſervations in that meaſure. Poſition 4° 9′ n. preceding.

11. κ Bootis, FL. 17. Trium in ſiniſtro manu præcedens.

Sept.27, 1779. Double. Very unequal. L. w.; S. d. Diſtance 12″503, a mean of the obſervations in 1779, 80, 81. Poſition about 30° ſ. preceding.

12. ι Orionis, FL. 44. Trium contiguarum in enſe auſtrina.

Oct. 7, 1779. Treble. It is the following or largeſt of the two ι's. One is L.; the other two are extremely ſmall. L. w.; the other two both duſky r. Diſtance of the neareſt 12″,5. Diſtance of the fartheſt 48″ 31‴. Poſition of the neareſt 43° 51′ following. Poſition of the fartheſt 11° 19′ ſ. following.

13. and 14. ι Orionis, FL. 44. Trium contiguarum in enſe auſtrina.

Oct. 7, 1779. Double-treble. It is the preceding or ſmalleſt of the two ι's. The preceding ſet (forming a triangle) conſiſts of three equal ſtars. All duſky r. Diſtance of the two neareſt, with 227, about 3 diameters. The following ſet (forming an arch) conſiſts of three ſtars of different ſizes. The middle ſtar is the largeſt; that to the ſouth is alſo pretty large; and the third is very ſmall. L. w.; l. w.; S. pale r. Diſtance 36″,25.

15. * μ Cygni, FL. 78.

Oct. 19, 1779. Double. Conſiderably unequal. L. w.; S. blueiſh. Diſtance 6″,927 mean meaſure. Poſition 20° 15′ ſ. following.

16. * In conſtellatione Delphini, FL. 1.

Nov. 15, 1779. Double. It is the ſtar ſouth preceding ε. A little unequal. Both w. Diſtance 12″,5. Poſition 9° 42′ ſ. preceding.

17. In extremitate caudæ Lacertæ, FL. 1.

Nov. 20, 1779, Double. Conſiderably unequal. L. w.; S. d. inclining to r. Diſtance 13″ 43‴ inaccurate. Poſition 76° 16′ ſ. preceding.

18. † γ Virginis, FL. 29. De quatuor in ala ſiniſtra, ſequens.

Jan. 21, 1780. Double. Equal. Both w. Diſtance 7″,333 mean meaſure. Poſition 40° 44′ ſ. following.

19. † ζ Cancri, FL. 16.

April 5, 1780. Double. Conſiderably unequal. L. pale r.; S. pale r. Diſtance 8″,046 mean meaſure. Poſition 88° 16′ ſ. preceding. See the 24th in the firſt claſs.

20. In conſtellatione Bootis.

June 25, 1780. Double. Draw a line through π and ζ to the ſmall ſtar under the right foot, and erecting a perpendicular towards the left foot of equal length, the end of it will mark out this double ſtar. Pretty unequal.

Both r. Diſtance 7″ 36‴ full meaſure. Poſition 59° 32′ n. preceding.

21. In conſtellatione Equulei, FL. 1.

Aug. 2, 1780. Double. Conſiderably unequal. L. w.; S. much inclining to r. Diſtance 9″,375 mean meaſure. Poſition 5° 39′ n. following. A third ſmall ſtar follows at ſome diſtance.

22. Quæ infra oculum Lyncis, FL. 12.

Aug. 7, 1780. Double. With 222, about 3 diameters of L. Conſiderably unequal. L. w.; S. pale r. Diſtance 9″ 23‴, not extremely accurate. Poſition 32° 33′ n. preceding. See the ſixth ſtar in the firſt claſs.

23. In conſtellatione Caſſiopeæ, FL. 34.

Aug. 8, 1780. Double. It is one of two teleſcopic ſtars, and is marked φ in HARRIS's maps. Extremely unequal. L. pale r.; S. d. Diſtance about 12″ or more.

24. θ Sagittæ, FL. 17.

Aug. 8, 1780. Treble. The two neareſt extremely unequal. L. pale r.; S. d. Third ſtar pale r. Diſtance of the two neareſt 11″ 8‴. Diſtance of the two largeſt 1′ 7″ 49‴.

25. In conſtellatione Serpentarii, FL. 39.

Aug. 24, 1780. Double. It is the moſt ſouth and largeſt of two in the finder. Very unequal. L. w.; S. inclining to blue. Diſtance 10″ 2‴, a little inaccurate. Poſition 87° 14′ n. preceding.

26. * In conſtellatione Cerberi I. HEVELII 1. FL. Herculis 95.

Sept. 8, 1780. Double. It is the ſtar in the leaf neareſt to Hercules's face and hand. Equal. Preceding w. Following

lowing blueiſh w. Diſtance 6″ 6‴. Poſition 4° 9′ ſ. preceding or n. following.

27. In conſtellatione Navis, near FL. 3.

Feb. 15, 1781. Double. It is a ſtar between η Canis majoris and ξ Navis. Equal. Diſtance about 15″.

28. In conſtellatione Navis, near FL. 9.

Feb. 15, 1781. Double. It is one of two teleſcopic ſtars under Monoceros. Diſtance about 8″.

29. In naribus Monocerotis, FL. 8. ::

Feb. 15, Double. Diſtance about 12″.

30. * In conſtellatione Leonis, FL. 54. Duarum ſupra dorſum ſequens.

Feb. 21, 1781. Double. Conſiderably unequal. L. brilliant w.; S. aſh-colour, or greyiſh w. Diſtance 7″ 6‴ mean meaſure. Poſition 9° 14′ ſ. following.

31. In conſtellatione Herculis.

May 20, 1781. Double. Over ι ::. Equal. Both very ſmall. Diſtance about 10″.

32. In conſtellatione Aquilæ, FL. 11.

July 25, 1781. Double. It is the moſt ſouth of two near ε and ζ. Exceſſively unequal. S. hardly viſible with 227, but pretty ſtrong with 460. Diſtance about 7″.

33. In conſtellatione Aquilæ, near FL. 7. and 8.

July 30, 1781. Double. It is a ſtar preceding the two ſmall ſtars north of *k* and *l*. Unequal. L. w.; S. blueiſh w. Diſtance 11″ 35‴ inaccurate, but not much.

34. In conſtellatione Aquarii, FL 94.

Aug. 20, 1781. Double. Between ψ and ω towards δ. Very unequal. Diſtance 13″ 45‴. L. pale r.; S. d.

35. In conſtellatione Serpentarii, FL. 54.

Aug. 21, Double. It is the preceding of two ſtars in the

1781. head. Exceſſively unequal. L. reddiſh w.; S. d. Diſtance about 8″.

36. In conſtellatione Perſei.

Sept. 14, 1781. Double. A little ſouth of *γ*. Conſiderably unequal. L. w.; S. w. inclining to r. Diſtance 11″ 53‴, rather full meaſure.

37. and 38. In conſtellatione Perſei, near FL. 38 ‡.

Sept. 24, 1781. Double-double. South preceding the firſt *o*. The equal ſet with 227, about 4 or 5 diameters. The unequal ſet about 5 or 6 diameters. Near this laſt ſet is alſo a third ſtar forming an obtuſe angle with the ſtars of this ſet. Diſtance about 10″.

39. *o* Perſei, FL. 40.

Sept. 24, 1781. Double. It is the ſecond or moſt northern *o*. Extremely unequal. L. w.; S. d. With 227, S. is hardly viſible; with 460, it appears at firſt ſight. Diſtance 14″ 59‴, inaccurate on account of the obſcurity of S.

40. In conſtellatione Herculis, near FL. 87.

Oct. 10, 1781. Double. Of three ſtars, forming an obtuſe angle, whereof FL. 87. (a ſtar ſouth of *μ*) is at the angular point, that towards Ramus Cereb. Extremely unequal. L. w.; S. d. Diſtance 10″ 20‴. Poſition 19° 37′ ſ. following.

41. * *i* Herculis, FL. 43.

Oct. 10, 1781. Double. Equal. Preceding ſtar w. A little inclined to r. Following w. Diſtance 11″ 43‴. Poſition 88° 23′ n. following.

42. In conſtellatione Trianguli.

Oct. 10, 1781. Double. It is a ſtar north following *δ*. Unequal. L. reddiſh. S. blueiſh. Both d. Diſtance about 6 or 7″.

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‡ Mr. BRYANT of Bath firſt obſerved theſe ſtars.

43. In ſiniſtro anteriore pede Monocerotis.

Oct. 20, Double. It is the moſt ſouth of two teleſcopic
1781. ſtars preceding the treble ſtar. Extremely unequal.
L. w.; S. d. Poſition 23^{d} 39′. n. preceding.

44. In ore Monocerotis.

Oct. 20, Double. Conſiderably unequal. L. w.; S. r.
1781. Diſtance 12″ 30‴. Poſition 60° 14′ n. following.

45. In conſtellatione Tauri, near FL. 10.

Oct. 22, Double. It is near the ſtar ſub pede et ſcapula
1781. dextra. Extremely unequal. L. pale r.; S. d. Poſition 35° 33′ ſ. preceding.

46. In conſtellatione Monocerotis.

Oct. 22, Double. It is the ſtar following the tip of the
1781. ear.

FOURTH CLASS OF DOUBLE STARS.

1. α Urſæ minoris, FL. 1. Stella Polaris.

Aug. 17, Double. Extremely unequal. L. w.; S. r.
1779. Diſtance 17″ 15‴. Poſition 66° 42′ ſ. preceding.

2. * η Lyræ, FL. 20. Duarum contiguarum ad ortum a teſta, borea.

Aug. 29, Double. Conſiderably unequal. L. w.; S. r.
1779. Diſtance 25″ 42‴. Poſition 31° 51′ ſ. preceding.
Three other ſtars in view.

3. ξ Capricorni, FL.

Sept. 19, Double. It is the preceding ſtar of two. Ex-
1779. tremely unequal. Diſtance about 25″.

4. η Perſei, I. HEVELII 9. In dextro brachio.

Sept. 20, 1779. Double. Very unequal. L. r.; S. blue. Diſtance 26″, very inaccurate. Poſition 20° 5′ n. preceding.

5. In conſtellatione Arietis, FL. 33. Quatuor inform. ſup. dorſ. præc.

Sept. 27, 1779. Double. It is the firſt in the head of the fly. L. w.; S. d. Conſiderably unequal. Diſtance 25″ 32‴ inaccurate. Poſition 87° 14′.

6. + θ Serpentis, FL. 63. In extremitate Caudæ.

Oct. 17. Double. Equal. Both w. Diſtance 19″,375.

7. ψ Draconis, FL. 31. Prima ad ψ.

Oct. 19, 1779. Double. Pretty unequal. L. w.; ſ. pale r. Diſtance 28″ 14‴.

8. * ζ Piſcium, FL. 86. Trium in lino lucidarum ſequens.

Oct. 19, 1779. Double. Pretty unequal. L. w.; S. w. inclining to blue. Diſtance 22″,187, not very accurate. Poſition 22° 37′ n. following.

9. * Prima ad ψ Piſcium, FL. 74. Trium in pinna coſtarum præcedens.

Oct. 30, 1779. Double. Diſtance 27″,5. Poſition about 80° ſ. following. An obſcure ſtar alſo within 1½ minute.

10. χ Tauri, FL. 59. Auſtralis ſequentis lateris quadrilateri, in cervice.

Oct. 30. Double. Diſtance 18″,75, very inaccurate.

11. χ Cygni, FL. 17.

Nov. 20. 1779. Double. Very unequal. L. w.; S. duſky r. Diſtance 24″ 52‴.

12. * ψ Aquarii, FL. 91.

Nov. 26, 1779. Double. It is the firſt of three ψ's. Unequal. Diſtance 23″ 5‴, pretty accurate.

13. In conſtellatione Leonis, FL. 83.

April 6, 1780. Double. It is a ſmall ſtar north preceding τ. A little unequal. Both inclining to r. Diſtance 29″ 5‴. Poſition 54° 55′ ſ. following.

14. In conſtellatione Aquilæ, FL 57.

Aug. 2, 1780. Double. It is the preceding of two, near the ſouth end of Antinous's bow. A little unequal. L. w.; S. w. inclining to r. Diſtance 29″ 28‴, pretty accurate. Poſition 81° 55′ ſ. preceding.

15. In dextra aure Camelopardali. I. HEVELII ultima.

Aug. 2, 1780. Double. A little unequal. L. reddiſh w.; S. reddiſh w. Diſtance 30″ 5‴.

16. In conſtellatione Caſſiopeæ, FL. 31.

Aug. 2, 1780. Double. It is marked with the letter A in HARRIS's maps. Diſtance about 20″ or more.

17. * Cor Caroli, FL. 12. Canum Venaticorum.

Aug. 7, 1780. Double. Very unequal. L. w.; S. inclining to r. Diſtance 20″ 0‴, inaccurate. Poſition 41° 47′ ſ. preceding.

18. * In conſtellatione Cygni, FL. 61.

Sept. 20. 1780. Double. It is a ſtar preceding τ. Pretty unequal. L. pale r.; S. r.; or L. r.; S. garnet. Diſtance 16″ 7‴. Poſition 36° 28′ n. following.

19. In conſtellatione Aurigæ, FL. 14.

Sept. 24, 1780. Double. It is the preceding ſtar of a cluſter of ſtars that precede φ and χ. Very unequal. L. reddiſh w.; S. d. Diſtance 16″ 8‴, a little inaccurate. Poſition 37° 38′ ſ. preceding.

20. ο Draconis, FL 47.

Oct. 3, 1780. Double. Very unequal. L. pale r.; S. duſky r. Diſtance 26″ 39″. Poſition 90° n. preceding or following, by exact eſtimation.

21. ζ Orionis, FL. 50. Trium in cingulo ſequens.

Oct. 10, 1780. Double. Very unequal. L. w.; S. d. Diſtance about 25″. Poſition 83° 25′ n. following, very inaccurate.

22. *f* Cygni, FL. 63. ::

Oct. 27, 1780. Double. Extremely unequal. L. fine w.; S. d. Diſtance 18″ 11‴.

23. 2 ad ω Cygni, FL. 45. In genu dextro.

Oct. 27, 1780. Double. Conſiderably unequal. L. reddiſh w.; S. d. Diſtance within 30″. Poſition 7° 23′ n. preceding.

24. 3 ad ω Cygni, FL. 46. In genu dextro.

Oct. 27, 1780. Treble. Very unequal, and extremely unequal. L. fine garnet; S. r.; ſmalleſt d. All within 30″. Poſition of the brighteſt of the two ſmall ſtars 44° 19′ n. preceding. Poſition of the fainteſt —— preceding.

25. In conſtellatione Ceti.

Dec. 23, 1780. Double. It is a ſtar near the place of the periodical ſtar *o*. Diſtance 16″,875, a little inaccurate.

26. In conſtellatione Navis, FL. 19. ::

Feb. 15, 1781. Double. It is a ſtar under the ham of Monoceros's right-foot. Diſtance about 25″.

27. In conſtellatione Comæ Berenices, FL. 24.

Feb. 28, 1781. Double. Conſiderably unequal. L. whitiſh r.; S. blueiſh r. Mean diſtance 18″ 24‴. Poſition 3° 28′ n. preceding.

28. In conſtellatione Geminorum.

Mar. 13, 1781. Double. It is near γ towards ζ Tauri. A little unequal. Both r. Diſtance 11″ 41‴. Poſition 57° 0′ ſ. preceding.

29. *b* Urſæ majoris, FL. 23. Duarum in collo ſequens.

Apr. 25, 1781. Double. Extremely unequal. L. reddiſh w.; S. d. Diſtance with 460, 19″ 14‴. Poſition 3° 14′ n. preceding.

30. In conſtellatione Lyncis, FL. 44.

May 26, 1781. Double. It is the eye or noſe of Leo minor. Unequal. Diſtance 24″ 53‴ inaccurate.

31. In conſtellatione Cephei, near FL. 27.

May 27, 1781. Treble. It is a ſtar near δ. Diſtance of the neareſt about 20″.

32. * In conſtellatione Serpentarii, FL. 61.

July 15, 1781. Double. It is a ſtar near γ. A little unequal. L. w.; S. grey. Diſtance 19″ 4‴, inaccurate. Poſition almoſt directly following.

33. In conſtellatione Aquilæ.

July 19, 1781. Treble. It is the firſt of two ſtars preceding ν. Diſtance of the two neareſt 21″ 59‴, inaccurate.

34. In conſtellatione Aquilæ, near FL. 64.

July 25, 1781. Double. It is near a ſtar preceding θ. Equal diſtance about 30″.

35. β Delphini, FL. 6. Auſtrina præcedentis lateris quadrilateri.

Aug. 1, 1781. Double. Extremely unequal. Hardly viſible with 227; pretty ſtrong with 460. Diſtance 25″ 54‴, rather narrow meaſure. Poſition 78° n. preceding, by exact eſtimation.

36. β Serpentis, FL. 28. In eductione colli.

Aug. 13, 1781. Double. Extremely unequal. L. w.; S. extremely faint. Diſtance 24″, pretty exactly eſtimated. Poſition 3 or 4° ſ. preceding, too obſcure for meaſuring.

37. δ Equulei, FL. 7. Duarum in ore ſequens.

Aug. 13, 1781. Double. Exceſſively unequal. S. hardly viſible with 227; but with 460, viſible at firſt ſight. L. w.; S. d. Diſtance 19″ 32‴. S. too obſcure to be very accurate. Poſition 11° 39′ n. following.

38. In conſtellatione Aquarii, FL. 24.

Aug. 14, 1781. Double. It is the ſtar in the cheek or hair of the neck. Very unequal. L. w.; S. d. Diſtance 25″, very inaccurate.

39. In conſtellatione Cygni.

Oct. 1, 1781. Double. It is a ſtar north following σ. Extremely unequal. L. w.; S. d. Diſtance 18″ exact eſtimation. Poſition 30° 28′ ſ. following.

40. α Trianguli, FL. 10.

Oct. 8, 1781. Double. It is the preceding of three teleſcopic ſtars. Unequal. Diſtance 17″ 19‴, pretty accurate.

41. μ Herculis, FL. 86.

Oct. 10, 1781. Double. Exceſſively unequal. The ſmall ſtar is not viſible with 227, nor with 278. I ſaw it very well with 460. L. inclined to pale r.; S. d. Diſtance, by pretty exact eſtimation, 18″. Poſition, by very exact eſtimation, 30° ſ. preceding.

42. In conſtellatione Herculis.

Oct. 10, 1781. Double. It is a ſtar juſt by ν. Conſiderably unequal. L. inclined to r.; S. inclined to blue. Diſtance 18″ 19‴. Poſition 4° 58′ n. preceding.

43. λ Eridani, FL. ultima. In origine fluvii.

Oct. 22, 1781. Double. It is the middle of three teleſcopic ſtars. Very unequal. L. w.; S. r.

44. In conſtellatione Tauri, near FL. 4.
Dec. 22, Double. It is a ſmall teleſcopic ſtar ſouth fol-
1781. lowing *s*. Extremely unequal. L. w.; S. d.

FIFTH CLASS OF DOUBLE STARS.

1 δ Herculis, FL. 11. In ſiniſtro humero.
Aug. 9, Double. Extremely unequal. L. w.; S. inclin-
1779. ing to r. Diſtance 33″,75. Poſition 72° 28′ ſ.
following.

2. * ζ Lyræ, FL. 6.
Aug. 29, Double. Pretty unequal. L. w.; S. w. inclining
1779. to pale roſe colour. Diſtance 41″ 58‴, perhaps a
little inaccurate. Poſition 62° 18′ ſ. following, a
little inaccurate.

3. * β Lyræ, FL. 10. Duarum in jugimento borea.
Aug. 29, Quadruple. All w. Firſt and ſecond conſiderably
1779. unequal. Firſt and third very unequal. Firſt and
fourth very unequal. The ſecond a little inclining
to r. The third and fourth more inclining to r.
Diſtance of the firſt and ſecond 43″, 57‴. Poſition
60° 28′ ſ. following, a little inaccurate.

4. δ Cephei, FL. 27. Sequitur tiaram.
Aug. 31, Double. Conſiderably unequal. L. reddiſh w.;
1779. S. blueiſh w. Diſtance 38″ 18‴, a bright object.

5. † β Cygni, FL. 6. In ore.
Sept. 12, Double. Conſiderably unequal. L. pale r.; S. a
1779. beautiful blue. The eſtimation of the colours the
ſame

ſame with 227 and 460. Diſtance 39″ 32‴, pretty accurate. Poſition 36° 28′ n. following.

6. * ν Scorpii, FL. 14. Duarum adjacentium boreæ frontis, borea.

Sept. 19, 1779. Double. Very unequal. Both w. Diſtance 38″ 20‴, pretty accurate. Poſition 69° 28′ n. preceding.

7. μ Sagittarii, FL. 13. In ſummo arcu, borealis.

Sept. 19, 1779. Treble. Two ſmall ſtars near on each ſide. L. w.; S. both r. Diſtance of the neareſt about 30″. Poſition — preceding, the other — following.

8. κ Herculis, FL. 7. In dextri brachii ancone.

Sept. 20, 1779. Double. A little unequal. L. r.; S. garnet; or L. pale r.; S. r. (when the ſtars are low the firſt eſtimation of the colours will take place). Diſtance 39″ 59‴. Poſition 79° 37′ n. following. Has alſo a third ſtar.

9. ι Bootis, FL. 21. Trium in ſiniſtra manu, media.

Sept. 27. 1779. Double. Very unequal. L. w.; S. d. Diſtance 37″,56. This is not a mean of the meaſures; for I ſuſpect a motion in one of the ſtars, which another year or two may ſhew. Poſition 52° 51′ n. following.

10. * δ Orionis, FL. 34. Trium in cingulo præcedens.

Oct. 6, 1779. Double. Conſiderably unequal. L. w.; S. blueiſh r. Diſtance 52″,968 full meaſure. Poſition 88° 10′ n. preceding.

11. † ν Draconis, FL. 24. and 25, In ore duplex.

Oct. 19, 1779. Double. A little unequal. L. pale r.; S. pale r. Diſtance 54″ 48‴. Poſition 44° 19′ n. preceding.

From the right aſcenſion and declination of theſe ſtars in FLAMSTEAD's catalogue we gather, that in his time their diſtance was 1′ 11″,418; their poſition 44° 23′ n. preceding; their magnitude equal or nearly ſo. The difference in the diſtance of the two ſtars is ſo conſiderable, that we can hardly account for it otherwiſe than by admitting a proper motion in either one or the other of the ſtars, or in our ſolar ſyſtem; moſt probably neither of the three is at reſt.

12. * λ Arietis, FL. 9. In vertice.

Oct. 30, 1779. Double. Conſiderably unequal. L. pale r.; S. duſky garnet. Diſtance 36″ 44‴, a little inaccurate. Poſition 42° 0′ n. following.

13. φ Tauri, FL. 52. Borea ſequentis lateris quadrilateri in Cervice.

Oct. 30. Double. Diſtance 55″,625, inaccurate.

14. In conſtellatione Monocerotis.

Dec. 5, 1779. Multiple. It is a ſpot over the right fore-foot; 4 or 5 ſmall ſtars within one minute.

15. *c* Urſæ majoris, FL. 16.

May 2, 1780. Double. Very unequal. L. whitiſh r.; S. d. Diſtance with 460, 48″ 59‴. Poſition 80° 47′ ſ. preceding.

16. σ Piſcium, FL. 76. Duarum in ore piſcis ſequentis borealior.

Aug. 3, 1780. Double. Extremely unequal. L. pale r.; S. duſky r. Diſtance 48″,125, pretty accurate. Poſition 15° 28′ n. preceding.

17. π Andromedæ, FL. 29. In dextro humero.

Aug. 25, 1780. Double. Extremely unequal. L. w.; S. blueiſh. Diſtance 34″ 12‴, inaccurate.

18. α Caſſiopeæ, FL. 18. In pectore.
Aug. 31, Double. Extremely unequal. L. pale r.; S. d.
1780. Diſtance 52″,812. Poſition 40° 58′ n. preceding.

19. γ Herculis, FL. 20. In dextro brachio.
Sept. 4, Double. Extremely unequal. L. reddiſh w.; S.
1780. r. Diſtance 41″ 49‴, a little inaccurate. Poſition
19° 30′ ſ. preceding.

20. ε Pegaſi, FL. 1.
Sept. 8, Double. Very unequal. L pale r.; S. d.; Diſ-
1780. tance 37″ 5‴, pretty accurate. Poſition 38° 19′ n.
preceding.

21. τ Aurigæ, FL. 29.
Sept. 26, Double, about 30″.

22. λ Aurigæ, FL. 15.
Sept. 30. Multiple. Two are within about 30″.

23. In conſtellatione Orionis.
Oct. 10, Double. It is a ſtar following *f*. Diſtance about
1780. 40″

24. In conſtellatione Ceti, FL. 37.
Oct. 12. Double. It is a ſtar between η and θ towards the
1780. north. Diſtance 42″,812, inaccurate.

25. τ Orionis, FL. 20. ſupra talum in tibia.
Oct. 23, Double. Very unequal. Diſtance about 30″

26. *b* Leonis, FL. 6.
Feb. 21, Double. Very unequal. L. r.; S. d. Diſtance
1781. 35″ 48‴. Poſition 12° 55′ n. following.

27. In conſtellatione Libræ, near FL. 31.
May 24, Double. The moſt ſouth of three ſmall ſtars in
1781. the finder. Equal, or the preceding rather the
largeſt. Both w. inclining to pale r. Diſtance
44″ 12‴, a little inaccurate. Poſition 40° 17′ ſ.
following.

28. In conſtellatione Cephei.

May.27, 1781. Double. It is a ſtar near β. Extremely unequal. Diſtance about 30″.

29. ν Serpentis, FL. 53. Poſt dextrum femur Serpentarii.

July 16. Double. Unequal. Diſtance about 35″.

30. In conſtellatione Serpentarii, FL. 53.

July 19, 1781. Double. It is a ſtar between α and β one-third of the way from α. Very unequal. L. w.; S. inclining to r. Diſtance 32″ 21‴, narrow meaſure.

31. In conſtellatione Aquilæ.

July 19, 1781. Double. It is the ſtar next but one preceding δ. Very unequal. L. r.; S. d. Diſtance about 30″.

32. α Andromedæ.

July 21, 1781. Double. Extremely unequal. The ſmall ſtar better with 460 than with 227. L. w.; S. d. Diſtance 55″ 32‴, rather narrow meaſure. Poſition 10° 37′ ſ. preceding.

33. *b* Aquilæ, FL. 15.

July.25, 1781. Double. Unequal. Both pale r. Diſtance 33″ 53‴, inaccurate.

34. In conſtellatione Aquilæ, near FL. 28.

July 25, 1781. Double. It is one of two ſtars near A. Diſtance about 35″.

35. In conſtellatione Aquilæ.

July 25. 1781. Double. It is a ſtar near that which follows θ. Very unequal. Diſtance about 40″.

36. *o* Scuti, FL. 2. in conſtellatione Aquilæ.

July 30, 1781. Double. Very unequal. L. pale r.; S. d. Diſtance 42″ 44‴, a little inaccurate.

37. υ Coronæ, FL. 18.

Sept. 21, 1781. Treble. Very unequal. L. w.; S. both r. Diſtance of the neareſt about 50″; the 1½ min. ‡

38. In conſtellatione Herculis, FL. 23.

Sept. 21, 1781. Double. It is the ſtar between ν and ξ Coronæ, the largeſt of a teleſcopic triangle. Diſtance 36″ 27‴, rather narrow meaſure. L. w.; S. w. inclining to r.

39. α Lyræ, FL. 3. In teſta fulgida.

Sept. 24. 1781. Double. Exceſſively unequal. By moon-light I could not ſee the ſmall ſtar with 278, and ſaw it with great difficulty with 460; but in the abſence of the moon I have ſeen it very well with 227. L. fine brilliant w.; S. duſky. Diſtance 37″ 13‴. Poſition 26° 46′ ſ. following.

Oct. 22, 1781. Having often meaſured the diameters of many of the principal fixed ſtars, and having always found that they meaſured leſs and leſs the more I magnified, I fixed upon this fine ſtar for taking a meaſure with the higheſt power I have yet been able to apply, and upon the largeſt ſcale of my new micrometer I could conveniently uſe. With a power of 6450 (determined by experiments upon a known object at a known diſtance) I looked at this ſtar for at leaſt a quarter of an hour, that the eye might adapt itſelf to the object; having experimentally found, that the aberration by this means will appear leſs and leſs, and, in the teleſcope I uſed upon this occaſion with powers from 460 to 1500, will often quite vaniſh, and leave a very well-defined circular diſk for the apparent diameter of the ſtars. The diameter of α Lyræ, by this attention, appeared perfectly round, and occaſionally ſeparated from rays that were flaſhing about it. From the very brilliant appearance of the ſtar with this great power, and a pretty accurate rough calculation founded on its apparent brightneſs, when obſerved with the naked eye with 227, with 460, with 6450, I ſurmiſe, that it has light enough to bear being magnified at leaſt a hundred thouſand times with no more than ſix inches of aperture, provided we could have ſuch a power, and other conſiderations would allow us to apply it. When I had as good a view as I expected to have, I took its diameter with my new micrometer upon a ſcale of eight inches and 4428 ten thouſandth to 1″ of a degree, and found it ſubtended an angle of 0″,3553. I had no perſon at the clock; but ſuppoſe the time of its paſſing through the field of my teleſcope (which in this great power is purpoſely left undefined, and as large as poſſible) was leſs than three ſeconds.

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‡ In a future collection the ſmall ſtar at the obtuſe angular point will be found as a double ſtar of the ſecond or third claſs.

40. ν Lyræ, FL. 8.

Sept.24, Treble. Extremely unequal. L. w.; S. both d.
1781. One n. preceding, the other ſ. following. Diſtance of the following ſtar 56″ 47‴, a little inaccurate. Poſition of the ſame 28° 27′ ſ. following.

41. A Perſei, FL. 43.

Sept.24. Double. Unequal. L. w. Diſtance about 50″.

42. In conſtellatione Lyræ.

Sept.25, Double. It is a ſmall ſtar juſt by η. A little unequal

1781. equal. Both r. Diſtance 38″ 8‴. Poſition 26° 18′
n. following.

43. In conſtellatione Cygni, FL. 76.

Oct. 1, Double. It is the third ſtar from ρ towards υ.
1781. Unequal. Diſtance 48″ by exact eſtimation. Poſition
—— preceding.

44. In conſtellatione Cygni, FL. 69.

Oct. 1, Treble. Very unequal. L. w.; S. both reddiſh.
1781. Poſition both —— preceding.

45. In conſtellatione Cygni.

Oct. 1, Double. It is the moſt ſouth of two teleſcopic ſtars
1781. following τ. Very unequal. L. w.; S. d. Diſtance
44″ by exact eſtimation. Poſition —— following.

46. *c* Cygni, FL. 16. 1^{a} ad *c*.

Oct. 5, Double. It is the ſtar next following θ. Almoſt
1781. equal. Both pale r. Diſtance 30″ by pretty exact
eſtimation.

47. *c* Cygni, FL. 26. 2^{a} ad *c*.

Oct. 8, Double. Very unequal. L. reddiſh w.; S. duſky
1781. r. Diſtance 39″ by pretty exact eſtimation.

48. * In conſtellatione Piſcium.

Oct. 8, Double. It is a teleſcopic ſtar juſt by θ northwards.
1781. Both d. Diſtance about 45″.

49. * In conſtellatione Arietis, FL. 30.

Oct. 15, Double. It is a ſmall ſtar over the Ram's back
1781. Nearly equal. Diſtance 31″ 6‴, inaccurate.

50. γ Leporis, FL. 13. In poſterioribus pedibus auſtrina.

Oct. 22, Double. Conſiderably unequal. Diſtance about 40′.

51. In conſtellatione Sagittæ.

Nov. 23, Double. It is a ſtar north following ε. Extremely
1781. unequal. Diſtance 32″ 48‴. L. r.; S. blue.

SIXTH CLASS OF DOUBLE STARS.

1. *ο* Ceti, FL. 68. In pectore nova.

Oct. 20. 1777. Double. Very unequal. L. garnet. S. duſky. Diſt. {mean of ſome very accurate meaſures 1′ 44″,218; mean of other very accurate meaſures 1′ 53″,032. As I can hardly doubt the motion of this ſtar, I have given the mean of the moſt accurate meaſures ſeparately; and hope in a few years time to be able to give a better account of it.

2. *ο* Serpentarii, FL. 67.

Aug. 29, 1779. Double. Diſtance about 1¼ min.

3. δ Lyræ, FL. 11.

Aug. 29, 1779. Double. Extremely unequal. L. w.; S. d. Diſtance about 4′, pretty exact eſtimation.

4. *α* Capricorni, FL. 5.

Sept. 19, 1779. Double. Very unequal. L. r.; S. d. Diſtance about 1¼ min. Poſition —— ſ. preceding.

5. In conſtellatione Arietis, FL. 35. ſupra dorſum.

Sept. 27, 1779. Double. It is the ſtar in the body of the fly. Diſtance 2′ 5″ 35‴.

6. *ε* Capricorni, FL. 39. Duarum in eductione caudæ præced.

Sept. 27, 1779. Double. Unequal. L. pale r. Diſtance about 1¼ min.

7. * *τ* Tauri, FL. 94. In eductione cornu borei.

Oct. 6. Double. Diſtance 1′ 11″,25‴, pretty accurate.

8. *κ* Tauri, FL. 59.

Oct. 6. Double. At a conſiderable diſtance.

9. * ζ Geminorum, FL. 43. In ſiniſtro genu ſequentis ♊^{1}.
Oct. 7, Double. Very unequal. L. reddiſh w.; S. duſky r.
1779. Diſtance 1′ 31″ 52‴, rather full meaſure. Poſition
81° 14′ n. preceding.

10. ο Cygni, FL. 31. Duarum in dextro pede ſequens.
Nov. 2, Double. Conſiderably unequal. L. pale r. S.
1779. blue. It is the following ſtar of the two ο's that are
cloſe together. Diſtance 1′ 39″ 57‴. Poſition
87° 14′ ſ. preceding.

11. * α Leonis, FL. 32. In corde.
Nov. 14, Double. Very unequal. L. w.; S. d. Diſtance
1779. 2′ 48″ 20‴. Poſition 30° 5′ n. preceding.

12. * τ Leonis, FL. 84. Quaſi in cubito.
April 6, Double. Conſiderably unequal. L. r.; S. inclining
1780. to blue. Diſtance 1′ 22″ 42‴. Poſition
73° 29′ ſ. following.

13. ο Leonis, FL. 95. In extremitate caudæ.
April 6, Double. Extremely unequal. L. reddiſh w.; S.
1780. d. Diſtance about 1½ min. Poſition about 80° n. following.

14. η Serpentis, FL. 58. In cauda.
June 19, Double. Extremely unequal. L. pale r.; S. d.
1780. Diſtance 1′ 21″ 2‴. Poſition 9° 7′ ſ. following.

15. In conſtellatione Bootis, near FL. 6.
June 25, Double. It is a teleſcopic ſtar near that which
1780. forms a rectangle with α and η. Diſtance about 2′.

16. δ Bootis, FL. 49. In dextro humero.
July 23, Double. Conſiderably unequal. Diſtance about
1780. 2½ min. L. reddiſh w.; S. w. Poſition 5° 46′ n.
following.

17. μ Bootis, FL. 51. In baculo recurvo.

July 30, 1780. Double. Unequal. Diſtance 2′ 8″, exact eſtimation. Poſition 80° 25′ ſ. following. L. reddiſh w. S. pale r. See the 17th ſtar of the firſt claſs.

18. ν Coronæ, FL. 21.

July 30, 1780. Double. Very unequal. L. r.; S. garnet. At ſome conſiderable diſtance. Poſition about 80° n. following.

19. χ Perſei.

Aug. 2, 1780. Multiple. An aſtoniſhing number of ſmall ſtars all within the ſpace of a few minutes. I counted not leſs than 40 within my ſmall field of view.

20. μ Perſei, FL. 51. Duarum in dextro poplite ſequens.

Aug. 2, Double. Very unequal. L. w. Diſtance about 1′½.

21. η Pegaſi, FL. 44.

Aug. 23. Double. Diſtance about 2¼ min.

22. In conſtellatione Draconis, I. HEVELII 69.

Aug. 7, 1780. Double. It is the ſtar between α Draconis and the tail of Urſa major. Diſtance about 3½ min.

23. In naribus Lyncis.

Aug. 7. Double. Diſtance about 2′.

24. *d* Caſſiopeæ, FL. 4.

Aug. 12, 1780. Treble. Two are large. Diſtance about 2′. A third is obſcure. Diſtance about 1¼ min. They form almoſt a rectangle.

25. In conſtellatione Caſſiopeæ, FL. 3.

Aug. 18. Double. Diſtance about 2¼ min.

26. ε Sagittæ, FL. 11.

Aug. 19, 1780, Double. Very unequal. L. r.; S. r. inclining to blue. Diſtance 1′ 31″ 53‴. Poſition 8° 32′ ſ. following.

27. In conſtellatione Aquilæ.

Aug. 24. Double. It is a ſtar north of θ. Diſtance
about 1′.

28. β Capricorni, FL. 9. Trium in ſequente cornu auſtrina.

Aug. 26, Double. Conſiderably unequal. Diſtance about
1780. 3′. Poſition —— preceding.

29. π Capricorni, FL. 10. Trium in roſtro præcedens.

Aug. 26. Double. Diſtance about 2½ min.

30. α Aurigæ, FL. 13. In humero ſiniſtro.

Sept. 8, Double. Extremely unequal. L. w.; S. d.
1780. Diſtance 2′ 49″ 8‴. Poſition 33° 42′ ſ. following.
With a power of 227, and my common micrometer,
the diameter of this ſtar meaſured 2″,5. The circumference
was remarkably well defined.

31. *d* Tauri, FL. 88. In ſiniſtro cubito.

Sept. 24, Double. Diſtance 1′ 10″,625. A little inac-
1780. curate.

32. λ Cygni, FL. 54.

Sept. 20, Double. Extremely unequal. L. blueiſh w.; S.
1780. d. Diſtance about 1 min. Poſition 12° 42′ ſ. following.

33. In conſtellatione Cygni, FL. 32.

Sept. 20. Double. Diſtance about 2 min.

34. θ Aurigæ, FL. 37. In dextro carpo.

Sept. 26. Double. Diſtance about 2½ min.

35. In conſtellatione Camelopardali, FL. 13.

Sept. 26. Double. It is the ſtar over the goat's head. Diſ-
1780. tance about 2′.

36. In conſtellatione Camelopardali, FL. 10.

Sept. 30. Double. Diſtance about 1½ min.

37. *c* Draconis, FL. 46. In flexura colli.

Oct. 3. Double. Diſtance 3 or 4″. A rich ſpot.

38. *e* Draconis, FL. 64 or 65.

Oct. 3. Double. Diſtance about 2′.

39. α Orionis, FL. 58. In dextro humero lucida rutilans.

Oct. 10, 1780. Double. Extremely unequal. L. r. but not deep; S. d. Diſtance 2′ 6″ 2‴. Poſition 62° 18′ ſ. following.

40. γ Leporis, FF. 13.

Feb. 21, 1781. Double. Diſtance about 2½ min.

41. ρ Cancri 5 ad ρ, FL. 67.

Feb. 21, 1781. Double. Very unequal. L. reddiſh w.; S. d. Diſtance 1′ 35″ 59‴. Poſition 50° 33′ n. preceding.

42. β Geminorum, FL. 78. In capite ſequentis ♊.

Mar. 13, 1781. Multiple. Extremely unequal. The neareſt diſtance 1′ 56″ 45‴, rather full meaſure. Poſition 24° 28′ n. following, not extremely accurate. This is the ſmalleſt. The next diſtance 3′ 17″ 19‴, pretty accurate. Poſition 15° 56′ n. following. A third I did not meaſure.

43. θ Virginis, FL. 51. De quatuor ultima et ſequens.

May 14, 1781. Double. Extremely unequal. L. w.; S. d. Diſtance 1′ 3″ 53‴, inaccurate. Poſition 24° 55′ n. preceding.

44. ι Libræ, FL. 24.

May 24, 1781. Double. Very unequal. L. w.; S. duſky r. Diſtance 1′ 5″ 10‴, not accurate. Poſition 22° 31′ ſ. following.

45. In conſtellatione Andromedæ.

July 21, 1781. Double. It is a ſtar near, towards α. L. r. Diſtance about 1½ min.

46.

46. α Aquilæ, FL. 53.

July 23, Double. Extremely unequal. L. w.; S. d. Diſ-
1781. tance 2′ 23″ 18‴. Poſition 64° 44′ n. preceding.

47. In conſtellatione Aquilæ, near FL. 35.

July 25, Double. It is one of the preceding ſtars of a
1781. ſmall quartile near *c*, not very near.

48. In conſtellatione Aquilæ, near FL. 35.

July 25, Double. It is alſo one of the preceding ſtars of a
1781. ſmall quartile near *c*, not very near.

49. In conſtellatione Aquilæ.

July 26. Double. The following ſtar of a trapezium near l.

50. In conſtellatione Aquilæ.

July 26, Double. The following ſtar of a trapezium near
1781, l. not near.

51. In monte Mænali Heveliana.

Aug. 5, Double. It is a ſtar near the middle. The fol-
1781. lowing of two, not very near.

52. In conſtellatione Bootis.

Aug. 17, Double. It is a ſtar between ε and *f*. Diſtance
1781. above 1′. Unequal.

53. In conſtellatione Bootis.

Aug. 17, Double. It is a ſtar more ſouth than *i*. Diſtance
1781. above 1′.

54. In conſtellatione Serpentarii.

Aug. 21, Double. It is a ſtar more ſouth than *o*. Diſtance
1781. 75″, exact eſtimation.

55. In conſtellatione Caſſiopeæ, FL. 2.

Sept. 6. Double. It is a ſtar near ε. L. r. Diſt. within 2′½.

56. θ Lyræ, FL. ultima.

Sept. 25, Double. Very unequal. L. w.; S. inclining to r.
1781. Diſtance about 1½ min. Poſition —— n. following.

57. In conſtellatione Cygni, FL. 79.

Oct. 1, Double. It is the fifth ſtar from ρ to υ. Unequal.
1781. L. w.; S. pale r. Diſtance 1′ 40″ eſtimation.

58. In conſtellatione Aquarii, FL. 5.

Oct. 5, Double. It is the moſt ſouth of two in the arrow
1781. of Antinous. Diſtance above 1′.

59. In conſtellatione Cygni, near FL. 28.

Oct. 5, Double. It is a ſtar near *h*. Diſtance 73″, exact
1781. eſtimation.

60. In conſtellatione Cygni.

Oct. 8, Double. It is a ſtar near the ſecond *c*. Conſi-
1781. derably unequal. L. w.; S. d. Diſtance 88″, exact
eſtimation.

61. In conſtellatione Piſcium, near FL. 7.

Oct. 8, Treble. It is a ſtar preceding *b*. They form a
1781. triangle, each ſide of which is about 1′.

62. κ Piſcium, FL. 8. In ventre.

Oct. 8, Double. Diſtance near 2′.

63. In conſtellatione Sagittæ.

Oct. 12, Double. It is near the ſtar north following ε.
1781. Extremely unequal. L. w. inclining to r.; S. d.
Diſtance 1′ 30″ 56‴. Poſition 4° 9′ ſ. preceding.
A third ſtar in the ſame direction, at a little more
than twice the diſtance. A fourth ſtar in view.

64. In conſtellatione Eridani.

Oct. 22, Double. It is the ſmall ſtar near κ. Diſtance
1781. about 1½ min.

65. In capite Monocerotis.

Oct. 22, Multiple. It is one ſtar with at leaſt 12 around it,
1781. all within the field of my teleſcope.

66. α Tauri, FL. 87. Splendida in auſtrina oculo.

Dec. 19, 1781. Double. Extremely unequal. L. r.; S. d. Diſtance 1′ 27″ 45‴. Poſition 52° 58′ n. following. With 460, the apparent diameter of this ſtar, when on the meridian, meaſured 1″ 46‴, a mean of two very compleat obſervations, they agreed to 6‴; with 932, it meaſured 1″ 12‴, alſo a mean of two excellent obſervations; they agreed to 8‴. The apparent diſk was perfectly well defined with both powers.

POSTSCRIPT TO THE CATALOGUE OF DOUBLE STARS.

SINCE my having delivered my paper on the Parallax of the Fixed Stars, in which I refer to the above Catalogue of Double Stars, I have received, by the favour of our Preſident Sir JOSEPH BANKS, the fourth volume of the Acta Academiæ Theodoro Palatinæ, which contains a moſt excellent Memoir of Mr. MAYER's, "De novis in Cœlo ſidereo Phænomenis;" wherein I ſee that the idea of aſcertaining the proper motion of the ſtars by means of ſmall ſtars that are ſituated at no great diſtance from large ones, has induced that gentleman before me to look out for ſuch ſmall ſtars. In the courſe of that undertaking he has diſcovered a good many double ſtars, of which he has given us a pretty large liſt, ſome of them the ſame with thoſe in my catalogue. My view being the annual parallax required ſtars much nearer than thoſe that would do for Mr.

Mayer's purpoſe; therefore I examined the heavens with much higher powers, and looked out chiefly for ſuch as were exceedingly cloſe.

The above catalogue contains 269 double ſtars, 227 of which, to my preſent knowledge, have not been noticed by any perſon. I hope they will prove no inconſiderable addition to the general ſtock, eſpecially as in that number there are a great many which are out of the reach of Mr. Mayer's and other mural quadrant or tranſit inſtruments. It can hardly be expected, that a power of 70 or 80 would be ſufficient to diſcover thoſe curious ſtars that are contained in the firſt claſs of my catalogue; ſo that it is not ſtrange they ſhould have intirely eſcaped Mr. Mayer's notice. We ſee that it is not for want of his looking at thoſe ſtars; for we find he has frequently obſerved ζ Cancri, the ſtar near Procyon, and the ſtar in Monoceros, without perceiving the ſmall ſtars near them, which I have pointed out. Nor is it only in the firſt claſs that his teleſcope wanted power, light, and diſtinctneſs; for the ſmall ſtars that are near β Orionis, β Serpentis, ζ Orionis, ε Pegaſi, α Lyræ, α Andromedæ, μ Sagittarii, α Aquilæ, η Pegaſi, δ Lyræ, ι Libræ, κ Piſcium, α Tauri, and many more, have eſcaped his diſcovery, though he has given us the places of other more diſtant ſmall ſtars not far from them, and therefore muſt have had them frequently in the field of view of his teleſcope. In ſettling the relative ſituations of very cloſe double ſtars, neither Mr. Mayer's inſtruments, nor his method, were adequate to the purpoſe. It is well known, that whenever we employ time as a meaſure, the reſults cannot be very accurate; becauſe a miſtake of no more than a tenth part of a ſecond in time will produce an error of a whole ſecond and an half in meaſure, ſo that his AR muſt be extremely defective. Nor could his micrometer give the declination much better unleſs the teleſcope had bore a power of at leaſt 4 or 500. When the angle of poſition is but ſmall, ſuch as 3, 4, 5, or 6 degrees, and the diſtance of the ſtars not above a few ſeconds, it is evident, that a micrometer muſt be able to meaſure tenths of a ſecond at leaſt to give even a tolerable exactneſs of poſition. On the contrary, the poſition being meaſured with ſuch a micrometer as I have conſtructed for the purpoſe, we may from thence deduce the declination, with great confidence, true to a quarter of a tenth of a ſecond for every ſecond of the diſtance of the ſtars.

Mr. MAYER's account of α Geminorum, for inſtance, gives a difference of 0″,7 of time in Æ, of 3″,8 in declination, and of 1 to 6 in magnitude or degree of light of the ſtars. Theſe quantities reduced to my notation, and compared with my meaſures of the ſame ſtar, give:

Mr. MAYER's	Diſtance 9″,633 from center to center	Mine	5″,156 diameters included.
	Poſition 23° 14′ n. preceding		32° 47′ n. preceding.
	Magnitude extremely unequal		A little unequal.

To account for this difference I aſcribe Mr. MAYER's error in diſtance to his method of meaſuring by time. The error of poſition follows always from an obſervation of the declination taken with the common micrometer, when it is deduced from an erroneous Æ. In my meaſures the diſtance and poſition are independent of each other, which I look upon as no ſmall advantage of my croſs-hair micrometer. The error in the magnitudes of the ſtars I aſcribe to the want of power in Mr. MAYER's teleſcope, which did not ſeparate the ſtars far enough for him to judge accurately of their ſize, otherwiſe he would ſoon have found, that inſtead of five there is hardly ſo much as one ſingle degree of difference in their magnitudes. See fig. 6. for a repreſentation of thoſe ſtars with my power of 460.

I do not mean to depreciate Mr. MAYER's method, the excellence of which is well known; and with ſome ſtars of my third, all thoſe of the fourth, fifth, and ſixth claſſes, as well as with thoſe ſtill farther diſtant, to which he has applied it with admirable ſkill, and "magno labore, multiſque "nocturnis vigillis" (as he very juſtly expreſſes himſelf) a better can hardly be wiſhed for; but with ſtars of the ſecond claſs which generally differ no more than one, two or three-tenths of a ſecond of time in Æ, and can never differ more than four tenths, the inſufficiency of meaſuring by time is obvious. In regard to the declination, it is alſo no leſs evident, that it is much more accurate to take an angle, which may be had true to 2 or 3° at moſt, than to meaſure its tangent, which in ſtars of the ſecond claſs is generally no more than 2, 3, or 4″ of a degree, and can never exceed five. I do not ſo much as mention the ſtars of the firſt claſs: they muſt certainly, as to ſenſe, paſs the meridian at the ſame inſtant of time. Their diſtance has even eluded the attacks of my ſmalleſt ſilk-thread micrometer armed with an excellent power of 460; but I ſhall ſoon apply my laſt new inſtrument to them *, not without hopes of ſucceſs. Now, though I have hitherto not been able to expreſs the diſtance of the ſtars of the firſt claſs, otherwiſe than by the proportion it bears to their apparent diameters, I think it a very great point gained, that one of my inſtruments at leaſt (*viz.* the croſs-hair micrometer) has laid hold of them: for their angle of poſition, I think, is within a very ſmall quantity as well determined as it is in thoſe of the ſecond claſs. This ſimple but moſt uſeful inſtrument can, by actual meaſure, diſcover beyond a doubt a motion in two ſtars that are very cloſe together, though it ſhould amount to no more than a tenth part of a ſecond of a degree, provided that motion be in ſuch a direction that the effect of it be thrown upon the angle of poſition; wherein, with ſome of the ſtars of the firſt claſs, it would occaſion an alteration of 10, 20, 30, or more degrees.

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* For a deſcription of which ſee p. 163.

I have marked all thoſe ſtars in my catalogue which have been obſerved by Mr. MAYER and other aſtronomers with an aſteriſk (*) affixed to the number that they may be known; thoſe with the mark of a dagger (†) have been obſerved by different aſtronomers before Mr. MAYER. Among the ſtars which are not marked, will be found ſeveral that have been obſerved by Mr. MAYER; but, on comparing them together, it will be ſeen, that they are obſervations of different ſmall ſtars; for inſtance, Mr. MAYER (Act. Acad. vol. IV. p. 296.) obſerved a ſmall ſtar near Rigel at the diſtance of 1′ 0″,5 Æ in time, and 2′ 55″,2 in difference of declination north preceding Rigel. In my ſecond claſs (the 34th ſtar) we alſo find Rigel; but the ſmall ſtar I have obſerved is one which has not been ſeen by Mr. MAYER, and is at a diſtance of no more than 6′ 27″. Poſition 68° 12′ ſouth preceding; and ſo on with other ſtars.

I have uſed the expreſſion *double-ſtar* in a few inſtances of the ſixth claſs in rather an extended ſignification: the example of FLAMSTEAD, however, will ſufficiently authorize my application of the term. I preferred that expreſſion to any other, ſuch as Comes, Companion, or Satellite; becauſe, in my opinion, it is much too ſoon to form any theories of ſmall ſtars revolving round large ones, and therefore I thought it adviſeable carefully to avoid any expreſſion that might convey that idea. I am

very

very well perſuaded, FLAMSTEAD, who firſt uſed the word Comes, meant it only in a figurative ſenſe.

I ſhall not fail to take the firſt opportunity of looking out for thoſe of Mr. MAYER's double-ſtars which I have not in my catalogue, amounting to 31; and alſo for one I find mentioned in *La Connoiſſance des Temps* for 1783, diſcovered by Mr. MESSIER.

XIII. *Deſcription of a Lamp-Micrometer, and the Method of uſing it. By Mr.* William Herſchel, *F. R. S.*

Read January 31, 1782.

THE great difficulty of meaſuring very ſmall angles, ſuch as hardly amount to a few ſeconds, is well known to aſtronomers. Since I have been engaged in obſervations on double ſtars, I have had ſo much occaſion for micrometers that would meaſure exceeding ſmall diſtances exactly, that I have continually been endeavouring to improve theſe inſtruments.

The natural imperfections of the parallel wire micrometer in taking the diſtance of very cloſe double ſtars are the following. When two ſtars are taken between the parallels, the diameters muſt be included. I have in vain attempted to find lines ſufficiently thin to extend them acroſs the centers of the ſtars ſo that their thickneſs might be neglected. The ſingle threads of the ſilk-worm, with ſuch lenſes as I uſe, are ſo much magnified that their diameter is more than that of many of the ſtars. Beſides, if they were much leſs than they are, the power of deflection of light would make the attempt to meaſure the diſtance of the centers this way fruitleſs: for I have always found the light of the ſtars to play upon thoſe lines and ſeparate their apparent diameters into two parts. Now ſince the ſpurious diameters of the ſtars thus included, to my certain knowledge, are continually changing according to the ſtate of the air, and the length of time we look at them, we are, in ſome reſpect, left at an uncertainty, and our meaſures taken at different times, and with different degrees of attention, will vary on that account. Nor can we come at the true diſtance of the centers of any two ſtars, one from another, unleſs we could tell what to allow for the ſemi-diameters of the ſtars themſelves; for different ſtars have different apparent diameters, which, with a power of 227, may differ from each other (as I have experienced) as far as two ſeconds.

The next imperfection is that which ariſes from a deflection of light upon the wires when they approach very near to each other; for if this be owing to a power of repulſion lodged at the ſurface, it is eaſy to underſtand, that ſuch powers muſt interfere with each other, and give the meaſures larger in proportion than they would have been if the repulſive power of one wire had not been oppoſed by a contrary power of the other wire.

Another very conſiderable imperfection of theſe micrometers is a continual uncertainty of the real zero. I have found, that the leaſt alteration in the ſituation and quantity of light will affect the zero, and that a change in the poſition of the wires, when the light and other circumſtances remain unaltered, will alſo produce a difference. To obviate this difficulty, whenever I took a meaſure that required the utmoſt accuracy, my zero was always taken immediately after, while the apparatus remained in the ſame ſituation it was in when the meaſure was taken; but this enhances the difficulty becauſe it introduces an additional obſervation.

The next imperfection, which is none of the ſmalleſt, is that every micrometer that has hitherto been in uſe requires either a ſcrew or a divided bar and pinion to meaſure the diſtance of the wires or divided image. Thoſe who are acquainted with works of this kind are but too ſenſible how difficult it is to have ſcrews that ſhall be perfectly equal in every thread or revolution of each thread; or pinions and bars that ſhall be ſo evenly divided as perfectly to be depended upon in every leaf and tooth to perhaps the two, three, or four thouſandth part of an inch; and yet, on account of the ſmall ſcale of thoſe micrometers, theſe quantities are of the greateſt conſequence; an error of a ſingle thouſandth part inducing in moſt inſtruments a miſtake of ſeveral ſeconds.

The laſt and greateſt imperfection of all is, that theſe wire micrometers require a pretty ſtrong light in the field of view: and when I had double ſtars to meaſure, one of which was very obſcure, I was obliged to be content with leſs light than is neceſſary to make the wires perfectly diſtinct; and ſeveral ſtars on this account could not be meaſured at all, though otherwiſe not too cloſe for the micrometer.

The inſtrument I am going to deſcribe, which I call a Lamp-Micrometer, is free from all theſe defects, and has, moreover, to recommend it, the advantage of a very enlarged ſcale. The conſtruction of it is as follows.

ABGCFE (fig. 1.) is a ſtand nine feet high, upon which a ſemi-circular board *qhogp* is moveable upwards or downwards, in the manner of ſome fire-ſcreens, as occaſion may require, and is held in its ſituation by a peg *p* put into any one of the holes of the upright piece AB. This board is a ſegment of a circle of fourteen inches radius, and is about three inches broader than a ſemi-circle, to give room for the handles *r*D, *e*P, to work. The uſe of this board is to carry an arm L, thirty inches long, which is made to move upon a pivot at the center of the circle, by means of a ſtring, which paſſes in a groove upon the edge of the ſemi-circle *pgohq*; the ſtring is faſtened to a hook at *o* (not expreſſed in the figure being at the back of the arm L), and paſſing along the groove from *ob* to *q* is turned over a pulley at *q*, and goes down to a ſmall barrel *e*, within the plane of the circular board, where a double-jointed handle *e*P commands its motion. By this contrivance we ſee the arm L may be lifted up to any altitude from the horizontal poſition to the perpendicular, or be ſuffered to deſcend by its own weight below the horizontal to the reverſe perpendicular ſituation. The weight of the handle P is ſufficient to keep the arm in any given poſition; but if the motion ſhould be too eaſy, a friction ſpring applied to the barrel will moderate it at pleaſure.

In front of the arm L a ſmall ſlider, about three inches long, is moveable in a rabbet from the end L towards the center backwards and forwards. A ſtring is faſtened to the left ſide of the little ſlider, and goes towards L, where it paſſes round a pulley at *m*, and returns under the arm from *m*, *n*, towards the center, where it is led in a groove on the edge of the arm, which is of a circular form, upwards to a barrel (raiſed above the plane of the circular board) at *r*, to which the handle *r*D is faſtened. A ſecond ſtring is faſtened to the ſlider, at the right ſide, and goes towards the center, where it paſſes over a pulley *n*, and the weight *w*, which is ſuſpended by the end of this ſtring, returns the ſlider towards the center when a contrary turn of the handle permits it to act.

a and *b* are two ſmall lamps, two inches high, 1¼ in breadth by 1¼ in depth. The ſides, back, and top, are made ſo as to permit no light to be ſeen, and the front conſiſts of a thin braſs ſliding door. The flame in the lamp *a* is placed three-tenths of an inch from the left ſide, three-tenths from the front, and half an inch from the bottom. In the lamp *b* it is placed at the ſame height and diſtance measuring from the right ſide. The wick of the flame conſiſts only of a ſingle very thin lamp-cotton thread; for the ſmalleſt flame being ſufficient it is eaſier to keep it burning in ſo confined a place. In the top of each lamp muſt be a little ſlit, lengthways, and alſo a ſmall opening in one ſide near the upper part, to permit air enough to circulate to feed the flame. To prevent every reflection of light, the ſide opening of the lamp *a* ſhould be to the right, and that of the lamp *b* to the left. In the ſliding door of each lamp is made a ſmall hole with the point of a very fine needle juſt oppoſite the place where the wicks are burning, ſo that when the ſliders are ſhut down, and every thing dark, nothing ſhall be ſeen but two fine lucid points of the ſize of two ſtars of the third or fourth magnitude. The lamp *a* is placed ſo that its lucid point may be in the center of the circular board where it remains fixed. The lamp *b* is hung to the little ſlider which moves in the rabbet of the arm, ſo that its lucid point, in a horizontal poſition of the arm, may be on a level with the lucid point in the center. The moveable lamp is ſuſpended upon a piece of braſs faſtened to the ſlider by a pin exactly behind the flame upon which it moves as a pivot. The lamp is balanced at the bottom by a leaden weight, ſo as always to remain upright, when the arm is either lifted above, or depreſſed below, the horizontal poſition. The double-jointed handles *r*D, *e*P, conſiſt of light deal rods, ten feet long, and the loweſt of them may have diviſions, marked upon it near the end P, expreſſing exactly the diſtance from the central lucid point in feet, inches, and tenths.

From this conſtruction we ſee, that a perſon at a diſtance of ten feet may govern the two lucid points, ſo as to bring them into any required poſition ſouth or north preceding or following, from 0 to 90° by uſing the handle P, and alſo to any diſtance from ſix-tenths of an inch to five or ſix and twenty inches by means of the handle D. If any reflection or appearance of light ſhould be left from the top or ſides of the lamps, a temporary ſcreen, conſiſting of a long piece of paſte-board, or a wire frame covered with black cloth, of the length of the whole arm and of any required breadth, with a ſlit of half an inch broad in the middle, may be affixed to the arm by four bent wires projecting an inch or two before the lamps, ſituated ſo that the moveable lucid point may paſs along the opening left for that purpoſe.

Fig. 2. repreſents part of the arm L, half the real ſize; S the ſlider; *m* the pulley, over which the cord *xtyz* is returned towards the center; *v* the other cord going to the pulley *n* of fig. 1. R the braſs piece moveable upon the pin *c*, to keep the lamp upright. At R is a wire rivetted to the braſs piece, upon which is held the lamp by a nut and ſcrew. Fig. 3. 4. repreſent the lamps *a*, *b*, with the ſliding doors open, to ſhew the ſituation of the wicks. W is the leaden weight with a hole *d* in it, through which the wire R of fig. 2. is to be paſſed when the lamp is to be faſtened to the ſlider S. Fig. 5. repreſents the lamp *a* with the ſliding door ſhut; *l* the lucid point; and *ik* the openings at the top, and *s* at the ſides for the admiſſion of air.

Every ingenious artiſt will ſoon perceive that the motions of this micrometer are capable of great improvement by the application of wheels and pinions, and other well known mechanical reſources; but, as the principal object is only to be able to adjuſt the two lucid points to the required poſition and diſtance, and to keep them there for a few minutes, while the obſerver goes to meaſure their diſtance, it will not be neceſſary to ſay more upon the ſubject.

I am now to ſhew the application of this inſtrument. It is well known to opticians and others, who have been in the habit of uſing optical inſtruments, that we can with one eye look into a microſcope or teleſcope, and ſee an object much magnified, while the naked eye may ſee a ſcale upon which the magnified picture is thrown. In this manner I have generally determined the power of my teleſcopes; and any one who has acquired a facility of taking ſuch obſervations will very ſeldom miſtake ſo much as one in fifty in determining the power of an inſtrument, and that degree of exactneſs is fully ſufficient for the purpoſe.

The Newtonian form is admirably adapted to the uſe of this micrometer; for the obſerver ſtands always erect, and looks in a horizontal direction, notwithſtanding the teleſcope ſhould be elevated to the zenith. Beſides, his face being turned away from the object to which his teleſcope is directed, this micrometer may be placed very conveniently without cauſing the leaſt obſtruction to the view: therefore, when I uſe this inſtrument I put it at ten feet diſtance from the left eye, in a line perpendicular to the tube of the teleſcope, and raiſe the moveable board to ſuch a height that the lucid point of the central lamp may be upon a level with the eye. The handles, lifted up, are paſſed through two loops faſtened to the tube, juſt by the obſerver, ſo as to be ready for his uſe. I ſhould obſerve, that the end of the tube is cut away ſo as to leave the left eye intirely free to ſee the whole micrometer.

Having now directed the teleſcope to a double ſtar, I view it with the right eye, and at the ſame time with the left ſee it projected upon the micrometer: then, by the handle P, which commands the poſition of the arm, I raiſe or depreſs it ſo as to bring the two lucid points to a ſimilar ſituation with the two ſtars; and, by the handle D, I approach or remove the moveable lucid point to the ſame diſtance of the two ſtars, ſo that the two lucid points may be exactly covered by, or coincide with the ſtars. A little practice in this buſineſs ſoon makes it eaſy, eſpecially to one who has already been uſed to look with both eyes open.

What remains to be done is very ſimple. With a proper rule, divided into inches and fortieth parts, I take the diſtance of the lucid points, which may be done to the greateſt nicety, becauſe, as I obſerved before, the little holes are made with the point of a very fine needle. The meaſure thus obtained is the tangent of the magnified angle under which the ſtars are ſeen to a radius of ten feet; therefore, the angle being found and divided by the power of the teleſcope gives the real angular diſtance of the centers of a double ſtar.

For inſtance, September 25, 1781, I meaſured *α* Herculis with this inſtrument. Having cauſed the two lucid points to coincide exactly with the ſtars center upon center, I found the radius or diſtance of the central lamp from the eye 10 feet 4,15 inches; the tangent or diſtance of the two lucid points 50,6 fortieth parts of an inch; this gives the magnified angle 35′, and dividing by the power 460, which I uſed, we obtain 4″ 34‴ for the diſtance of the centers of the two ſtars. The ſcale of the micrometer at this very convenient diſtance, with the power of 460 (which my teleſcope bears ſo well upon the fixed ſtars that for near a twelve-month paſt I have hardly uſed any other) is above a quarter of an inch to a ſecond; and by putting on my power of 932, which in very fine evenings is

extremely

extremely diſtinct, I obtain a ſcale of more than half an inch to a ſecond, without increaſing the diſtance of the micrometer; whereas the moſt perfect of my former micrometers, with the ſame inſtrument, had a ſcale of leſs than the two thouſandth part of an inch to a ſecond.

The meaſures of this micrometer are not confined to double ſtars only, but may be applied to any other objects that require the utmoſt accuracy, ſuch as the diameters of the planets or their ſatellites, the mountains of the moon, the diameters of the fixed ſtars, &c.

For inſtance, October 22, 1781, I meaſured the apparent diameter of α Lyræ; and judging it of the greateſt importance to increaſe my ſcale as much as convenient, I placed the micrometer at the greateſt convenient diſtance, and (with ſome trouble, for want of longer handles, which might eaſily be added) took the diameter of this ſtar by removing the two lucid points to ſuch a diſtance as juſt to incloſe the apparent diameter. When I meaſured my radius it was found to be twenty-two feet ſix inches. The diſtance of the two lucid points was *about* three inches; for I will not pretend to *extreme* nicety in this obſervation, on account of the very great power I uſed, which was 6450. From theſe meaſures we have the magnified angle 38′ 10″: this divided by the power gives 0″,355 for the apparent diameter of α Lyræ. The ſcale of the micrometer, on this occaſion, was no leſs than 8,443 inches to a ſecond, as will be found by multiplying the natural tangent of a ſecond with the power and radius in inches.

November 28, 1781, I meaſured the diameter of the new ſtar; but the air was not very favourable, for this ſingular ſtar was not ſo diſtinct with 227 that evening as it generally is with 460: therefore, without laying much ſtreſs upon the exactneſs of the obſervation, I ſhall only report it to exemplify the uſe of the micrometer. My radius was 35 feet 11 inches. The diameter of the ſtar, by the diſtance of the lucid points, was 2,4 inches, and the power I uſed 227: hence the magnified angle is found 19′, and the real diameter of the ſtar 5″,022. The ſcale of this meaſure ,474 milleſimals of an inch, or almoſt half an inch to a ſecond.

Fig. 4.

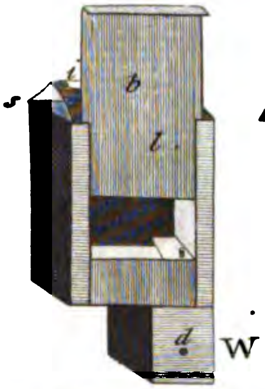


Fig. 3.

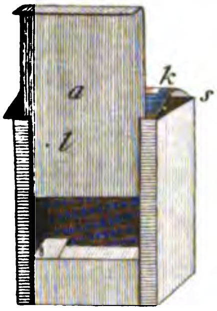
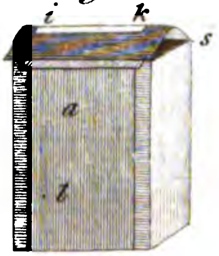


Fig. 5.



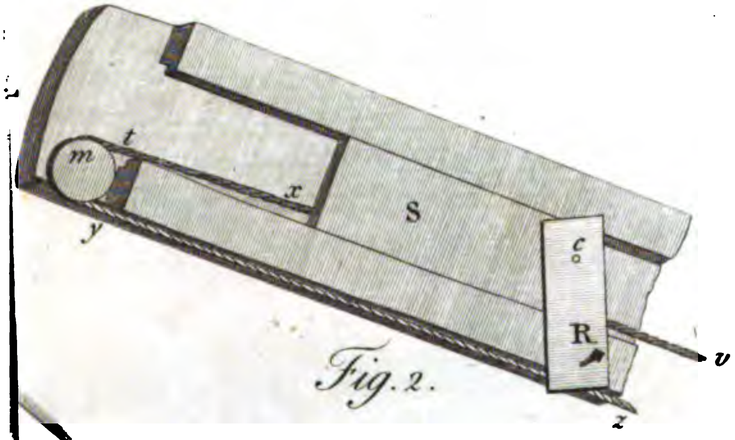
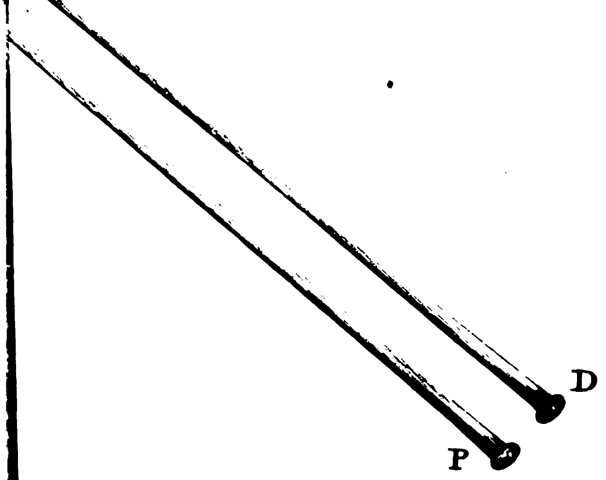


Fig. 2.



Basire S.

XIV. *A Paper to obviate ſome Doubts concerning the great Magnifying Powers uſed. By Mr.* Herſchel, *F. R. S.*

TO SIR JOSEPH BANKS, BART. F. R. S.

SIR,

I HAVE the honour of laying before you the reſult of a ſet of meaſures I have taken in order to aſcertain once more the powers of my Newtonian ſeven-feet reflector. The method I have formerly uſed, and which I ſtill prefer to that which I have now been obliged to practiſe, requires very fine weather and a ſtrong ſun-ſhiny day; but my impatience to anſwer the requeſts of Sir JOSEPH BANKS would not permit me to wait for ſo precarious an opportunity at this ſeaſon of the year. The difference in all the powers, as far as 2010, will be found to be in favour of thoſe I have mentioned; and, I believe, a much greater concurrence could not well be expected, where different methods of aſcertaining them are uſed. The variation in the two higheſt powers is more conſiderable than I was aware of; but ſtill may eaſily be ſhewn to be a neceſſary conſequence of the difference in the methods. However, if upon comparing together the methods it ſhould be thought, that the power 5786 is nearer the truth than 6450, I ſhall readily join to correct that number. The manner in which I have now determined the powers is as follows: I took one of the eye lenſes which magnifies leaſt, and meaſured its ſolar focus by the ſun's rays as exactly as I could five times, which proved to be 1.01, 1.04, 1.09, 1.01, 1.05, in half-inch meaſure, a mean of which is 1.04. The ſidereal focus of my ſeven-feet ſpeculum is 170.4 in the ſame meaſure. Thence, dividing 170.4 by 1.04 we find that the teleſcope will magnify 163.8 times when that lens is uſed. This power being found, I applied the ſame lens as a ſingle microſcope to view with it a certain object, which was a drawn braſs wire faſtened ſo as not to turn upon its axis or change its poſition; for theſe wires are ſeldom perfectly round, or of an even ſize, and it is therefore neceſſary to uſe this precaution to prevent errors: then, with a fine pair of compaſſes, I took four independent meaſures of the image of the braſs wire, which was thrown upon a ſheet of paper exactly 8½ inches from the lens, the eye being always as cloſe to the lens as poſſible. I viewed the ſame wire, exactly in the ſame manner, with every one of the lenſes, and meaſured the pictures upon the paper. When I came to the higher powers the wire was exchanged for another 4.37 times thinner than the former, as determined by comparing the proportion of their images 54 to 235¼, taken by the ſame lens.

When the images of theſe wires are obtained, the power of the teleſcope, with every one of the lenſes, becomes known by one plain analogy: *viz.* as the image of the wire by the firſt lens (77¾) is to the power it gives to the teleſcope (163.8), ſo is the image of the wire by the ſecond lens (119) to the power it will give to the ſame teleſcope (250.7). The particulars of all the meaſures are as follows:

Powers as they have been called in my papers.	Images of a wire thrown upon a paper in hundredths of half inches.				A mean of the four meaſures.	Powers as they come out by this method.
146	77	78	78	78	77¾	163.86 = $\frac{170.4}{1.04}$
227	119	119	119	119	119	250.7
278	143	143	144	143	143¼	301.8
460	236	236	235	236	235¾	496.7
	Smaller wire.					
	53	54	55	54	54	
754	83	85	84	85	84¼	775.1
932	107	107	107	108	107¼	986.7
1159	128	128	129	128	128¼	1179.9
1536	An excellent lens, loſt about eight months ago.					
2010	236	236	238	336	236½	2175.8
3168	281	283	281	280	281¼	2585.5
6450	635	625	630	626	629	5786.8

I beg leave, Sir, now to give a ſhort deſcription of the method I have formerly uſed to determine theſe powers. In the year 1776 I erected a mark of white paper, exactly half an inch in diameter, which I viewed with my teleſcope at the greateſt convenient diſtance with one of the leaſt magnifiers. An aſſiſtant was placed at rectangles in a field, at the ſame diſtance from my eye as the object from the great ſpeculum of the teleſcope. Upon a pole erected there I viewed the magnified image of the half inch, and the aſſiſtant marked it by my direction; this being meaſured gave the power of the inſtrument at once. The power thus obtained was corrected by theory, to reduce it to what it would be upon infinitely diſtant objects. The powers of the reſt of the lenſes I deduced from this by a *Camera-eye-piece*, which I made for that purpoſe. ABCD (fig. 1.) repreſents a perpendicular ſection of it. The end A ſcrews into the teleſcope. Upon the end B may be ſcrewed any of the common ſingle-lens eye-pieces. *lmn* is a ſmall oval plane ſpeculum, adjuſted to an angle of 45° by three ſcrews, two whereof appear at *op*. When the obſerver looks in at B, he may ſee the object projected upon a ſheet of paper on a table placed under the Camera-piece, and meaſure its picture *a*, *b*, as in fig. 2. The power of one lens therefore being known, that of the reſt was alſo found by comparing the meaſures of the projected images.

It may not be amiſs to mention ſome of the advantages and inconveniencies attending each of theſe methods. When we take the focus of an eye-lens, which the firſt method requires, we are liable to a pretty conſiderable uncertainty, and in very ſmall lenſes it is not to be done at all. Moreover, in calculating the power by that focus no account is made of the aberration which takes place in all ſpecula and lenſes, and increaſes the image, ſo that we rather find out how much the teleſcope *ſhould* magnify than how much it really *does* magnify; but in determining the power by an experiment we avoid theſe difficulties.

On the other hand, when the power is very great, the latter method becomes inconvenient, both on account of want of light in the object, and a very conſiderable aberration which takes place, and makes the picture too indiſtinct to be very accurate in the meaſure, and of courſe larger than it ought to be; and this will account for the exceſs in the meaſures of my two largeſt powers. However, when I employed 6450 upon the diameter of α Lyræ, I incline to think the method I had uſed when I determined that power, ought to be preferred, becauſe my Lamp-micrometer gives the meaſure of an object as it appears in the teleſcope, and therefore this aberration is included, and ſhould be taken into conſideration.

To prevent any miſtakes, I wiſh to mention again, that I have all along proceeded *experimentally* in the uſe of my powers, and that I do not mean to ſay I have uſed 6450 (or 5786) upon the planets, or even upon double ſtars; every power I have mentioned is to be underſtood as having been uſed juſt as it is related; but farther inferences ought not as yet to be drawn. For inſtance, my obſervations on ε Bootis mention that I have viewed that ſtar with 2010 (or as in the above table with 2175) extremely diſtinct; but upon ſeveral other celeſtial objects I have found this power of no ſervice. Many plauſible ſuggeſtions have already occurred to account for theſe appearances; but I wait till farther experiments ſhall have furniſhed me with more materials to reaſon upon. The uſe of high powers is a new and untrodden path, and in this attempt variety of new phænomena may be expected, therefore I wiſh not to be in a haſte to make general concluſions. I ſhall not fail to purſue this ſubject, and hope ſoon to be able to attack the celeſtial bodies with a ſtill ſtronger armament, which is now preparing.

It remains now only for me to make the moſt ſincere acknowledgement for the favours you have ſhewn to me, and to ſay that I ſhall ever remain, with equal reſpect and gratitude,

SIR, your moſt obedient, &c.

P. S. Dr. WATSON junior has done me the favour ſeparately to examine and meaſure the powers of my teleſcope; and placing the greateſt confidence in his accuracy, I rely on his meaſures at leaſt as much as my own.

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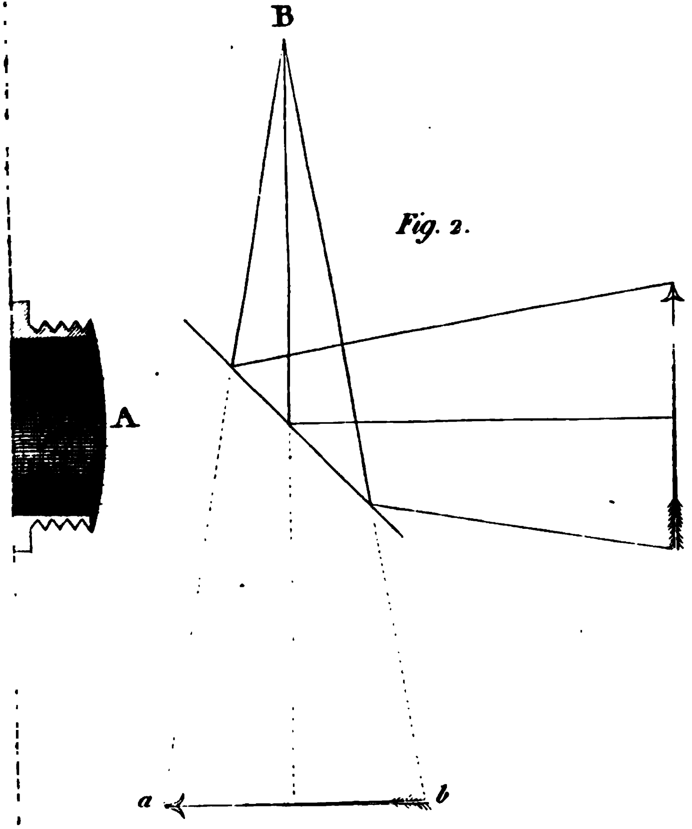
Fig. 2.



XIV. *Continuation of the Experiments and Obſervations on the Specific Gravities and Attractive Powers of various Saline Subſtances.* *By* Richard Kirwan, *Eſq. F. R. S.*

Read April 11, 1782.

BEFORE I enter into a detail of the new experiments I have made in the proſecution of this ſubject, I muſt beg leave to rectify ſome miſtakes I have fallen into in my laſt paper.

1. In computing the quantity of acid taken up by 10,5 gr. of mild vegetable fixed alkali, I made no allowance for the ſmall quantity of earth it contains, *viz.* 0,7035 of a grain; but in large quantities of alkali, this proportion is conſiderable, and occaſioned a ſmall but ſenſible error in my ſubſequent calculations of the proportion of ingredients in neutral ſalts, the quantity of alkali being, by that fraction, leſs than I ſuppoſed it in 10,5 gr. This correction being made, it will be found, that 100 gr. of perfectly dry vegetable fixed alkali (abſtracted from the quantity of earth) generally contain 22,457 gr. of fixed air inſtead of 21, as I before determined; yet the former determination is right, where the earth is not ſeparated, yet may well be ſuppoſed to exiſt, as in the alkali of pearl-aſh, purified by three repeated calcinations and ſolutions. Hence alſo 100 gr. of ſuch alkali, free from earth, water, and fixed air, take up 46,77 gr. of the mineral acids, that is, of the mere

acid

acid part; and 100 gr. of common mild vegetable alkali take up about 36,23 of real acid.

100 gr. of perfectly dry tartar vitriolate contain 30,21 of real acid, 64,61 of fixed alkali, and 5,18 of water. Cryſtallized tartar vitriolate loſes only 1 *per cent.* of water in a heat in which its acid alſo is not ſeparated in any degree, and therefore contains 6,18 of water.

100 gr. of nitre, perfectly dried, contain 30,86 of acid, 66 of alkali, and 3,14 of water; but in cryſtallized nitre the proportion of water is ſomewhat greater; for 100 gr. of theſe cryſtals, being expoſed to a heat of 180° for two hours, loſt 3 gr. of their weight, without exhaling any acid ſmell; but when expoſed to a heat of 200°, the ſmell of the nitrous acid is diſtinctly perceived. Hence 100 gr. of cryſtallized nitre contain 29,89 of mere acid, 63,97 of alkali, and 6,14 of water.

100 gr. of digeſtive ſalt perfectly dry contain 29,68 of marine acid, 63,47 of alkali, and 6,85 of water. 100 gr. of cryſtallized digeſtive ſalt loſe but 1 gr. of their weight before the ſmell of the marine acid is perceived; and hence they contain 7,85 gr. of water.

But the miſtake which coſt me moſt time and pains to correct was that which I fell into when I imagined, that the mixtures of oil of vitriol and water, and ſpirit of nitre and water, had attained their maximum of denſity when they had cooled to the temperature of the atmoſphere, which at the time I made my experiments ſtood between 50 and 60° of FAHRENHEIT. The former I had even ſuffered to ſtand ſix hours, which was much longer than was neceſſary for its cooling; but when the acid was ſo much diluted as to cauſe little or no heat, I allowed it to ſtand but for a very little time before I examined its denſity: yet ſeveral months after I found many of theſe mixtures much denſer than when I firſt examined them, and that at leaſt twelve hours reſt was requiſite before concentrated oil of ~~vitriol, to which even twice its weight of water~~ is added attains its utmoſt denſity, and ſtill more when a leſſer proportion of water is uſed: thus, when I made the mixture of 2519,75 gr. of oil of vitriol, whoſe ſpecific gravity was 1,819, with 180 of water, I found its denſity ſix hours after 1,771; but after twenty-four hours it was 1,798: and hence, according to the reaſoning in the former paper, the accrued denſity was at leaſt ,064 inſtead of ,045 as I had formerly found it. But by uſing oil of vitriol ſtill more concentrated, whoſe ſpecific gravity was 1,8846, I was enabled, by a ſimilar train of reaſoning, to make a ſtill nearer approximation, and found that the accrued denſity of oil of vitriol, whoſe ſpecific gravity is 1,819, amounts to 0,104; and conſequently its mathematical ſpecific gravity is 1,715. 6,5 gr. of this oil of vitriol contained, as I before found, 3,55 of mere acid, and the remainder water, then the weight of an equal bulk of water is 3,79 gr.; and ſubtracting from this the weight of the water that enters into the compoſition of the oil of vitriol, it will be found, that the weight of a bulk of water, equal to the acid part, is 0,84, and conſequently the ſpecific gravity of the pure and mere acid part is 4,226. Upon this ground, and conſtantly allowing the mixtures to reſt at leaſt twelve hours (until the oil of vitriol was diluted with four times its weight of water, and then often only ſix hours) before their denſity was examined, I conſtructed the table hereto annexed; the temperature of the room I conſtantly kept between 50 and 60°.

Oil

Oil of vitriol.	Acid.	Water.	Accrued denſity.	Mathemat. ſp. gravity.	
Grains.					
1000	– –	387,95	,07	1,877	1,846
1100	– –	487,95	,104	1,738	1,844
1200	– –	587,95	,105	1,637	1,742
1300	– –	687,95	,144	1,561	1,705
1400	– –	787,95	,144	1,500	1,644
1500	– –	887,95	,137	1,452	1,589
1600	– –	987,95	,137	1,412	1,539
1700	– –	1087,95	,130	1,379	1,509
1800	– –	1187,95	,124	1,350	1,474
1900	– –	1287,95	,116	1,326	1,442
2000	– –	1387,95	,116	1,304	1,420
2100	– –	1487,95	,112	1,286	1,398
2200	– –	1587,95	,112	1,269	1,381
2300	– –	1687,95	,108	1,254	1,362
2400	– –	1787,95	,104	1,241	1,345
2500	– –	1887,95	,104	1,229	1,333
2600	– –	1987,95	,101	1,219	1,320
2700	– –	2087,95	,096	1,209	1,307
2800	– –	2187,95	,091	1,200	1,291
2900	– –	2287,95	,090	1,192	1,282
3000	– –	2387,95	,090	1,184	1,274
3100	612,05	2487,95	,090	1,177	1,267
3200	– –	2587,95	,090	1,170	1,260
3300	– –	2687,95	,089	1,164	1,253
3400	– –	2787,95	,084	1,159	1,243
3500	– –	2887,95	,083	1,150	1,233
3600	– –	2987,95	,073	1,149	1,222
3700	– –	3087,95	,073	1,144	1,217
3800	– –	3187,95	,071	1,140	1,211
3900	– –	3287,95	,071	1,136	1,208
4000	– –	3387,95	,071	1,132	1,204
4100	– –	3487,95	,070	2,128	1,198
4200	– –	3587,95	,070	1,125	1,195
4300	– –	3687,95	,070	1,121	1,191
4400	– –	3787,95	,070	1,118	1,188
4500	– –	3887,95	,070	1,115	1,185
4600	– –	3987 95	,070	1,113	1,183
4700	– –	4087,95	,070	1,110	1,180
4800	– –	4187,95	,070	1,107	1,177
4900	– –	4287,95	,070	1,105	1,175
5000	– –	4387,95	,070	1,103	1,172
5100	– –	4487,95	,069	1,100	1,169

Oil

Oil of vitriol.	Acid.	Water.	Accrued denſity.	Mathemat. ſp. gravity.	
Grains.					
5200	– –	4587,95	,069	1,098	1,167
5300	– –	4687,95	,069	1,096	1,165
5400	– –	4787,95	,069	1,094	1,163
5500	– –	4887,95	,068	1,092	1,160
5600	– –	4987,95	,067	1,091	1,158
5700	– –	5087,95	,067	1,089	1,156
5800	– –	5187,95	,067	1,087	1,154
5900	– –	5287,95	,065	1,086	1,151
6000	– –	5387,95	,064	1,084	1,148
6100	612,05	5487,95	,064	1,082	1,146
6200	– –	5587,95	,063	1,081	1,144
6300	– –	5687,95	,062	1,080	1,142
6400	– –	5787,95	,062	1,078	1,140
6500	– –	5887,95	,061	1,077	1,138
6600	– –	5987,95	,060	1,076	1,136
6700	– –	6087,95	,060	1,074	1,134
6800	– –	6187,95	,060	1,072	1,132
6900	– –	6287,95	,060	1,070	1,130
7000	– –	6387,95	,059	1,069	1,128

With regard to the nitrous acid, I found alſo I had been a little too precipitate as to the time of examining its denſity after it had been mixed with water. Hence, making uſe of ſome whoſe ſpecific gravity was 1,474, I allowed the mixtures to reſt twelve hours, until it was diluted with twice its weight of water, and the ſubſequent mixtures ſix hours at leaſt; by the former proceſs of reaſoning, I found the ſpecific gravity of the mere nitrous acid to be 5,530.

Spirit of nitre.	Acid.	Water.	Accrued denſity.	Mathemat. ſp. gravity.	Phyſical ſp. gravity.
900	- -	507	- -	1,557	1,557
1000	- -	607	- -	1,474	1,474
1100	- -	707	,035	1,413	1,448
1200	- -	807	,056	1,367	1,423
1300	- -	907	,065	1,329	1,394
1400	- -	1007	,065	1,298	1,363
1500	- -	1107	,077	1,273	1,350
1600	- -	1207	,082	1,251	1,333
1700	- -	1307	,082	1,233	1,315
1800	- -	1407	,083	1,217	1,300
1900	- -	1507	,083	1,204	1,287
2000	- -	1607	,096	1,191	1,269
2100	- -	1707	,088	1,181	1,254
2200	- -	1807	,071	1,176	1,247
2300	- -	1907	,068	1,162	1,230
2400	- -	2007	,068	1,154	1,222
2500	- -	2107	,067	1,147	1,214
2600	- -	2207	,065	1,141	1,206
2700	- -	2307	,063	1,135	1,198
2800	- -	2407	,061	1,129	1,190
2900	- -	2507	,058	1,124	1,182
3000	- -	2607	,055	1,120	1,175
3100	393	2707	,054	1,116	1,170
3200	- -	2807	,054	1,111	1,165
3300	- -	2907	,053	1,108	1,161
3400	- -	3007	,052	1,104	1,156
3500	- -	3107	,050	1,101	1,151
3600	- -	3207	,048	1,098	1,146
3700	- -	3307	,047	1,095	1,142
3800	- -	3407	,045	1,092 or 3	1,137
3900	- -	3507	,043	1,089	1,132
4000	- -	3607	,040	1,087	1,127
4100	- -	3707	,037	1,085	1,122
4200	- -	3807	,035	1,083	1,118
4300	- -	3907	,034	1,080	1,114
4400	- -	4007	,032	1,078	1,110
4500	- -	4107	,029	1,077	1,106
4600	- -	4207	,027	1,075	1,102
4700	- -	4307	,025	1,073	1,098
4800	- -	4407	,022	1,072	1,094
4900	- -	4507	,020	1,070	1,090
5000	- -	4607	,018	1,068	1,086
5100	- -	4707	,015	1,067	1,082
5200	- -	4807	,012	1,066	1,078
5300	- -	4907	,008	1,066	1,074

The foregoing experiments were made at the temperature of between 50 and 60° of FAHRENHEIT; but as it may be ſuſpected, that the denſity of the above acids is conſiderably altered at degrees of temperature conſiderably different, I endeavoured to find the quantity of this alteration, and to calculate what this denſity would be at 55°, that the quantities of acid and water may thereby be inveſtigated.

To this end I took ſome dephlogiſticated ſpirit of nitre, and examined its ſpecific gravity at different degrees of heat, and found it as follows:

	Deg.		Sp. gravity.
at	30	-	1,4653
	46	-	1,4587
	86	-	1,4302
	120	-	1,4123

Therefore, the total expanſion of this ſpirit of nitre from 30 to 120°, that is, by 90° of heat, was 0,0527; for 1,4650 - 1,4123 = ,0527, by which we ſee that the dilatations are nearly proportional to the degrees of heat: for beginning with the firſt dilatation from 30 to 46°, that is, by 16° of heat ÷ 90 . 0,0527 :: 16 . 0,0093; but in reality theſe 16° of heat afforded a dilatation equal only to 0,0063; for 1,4650 - 1,4587 = 0,0063; ſo that the difference betwixt the calculated and obſerved dilatations is only $\frac{30}{10000}$, a difference of no conſequence in the preſent caſe, and that might ariſe from the immerſion of the cold glaſs ball filled with mercury in the liquor, it being the ſolid I uſe to try the ſpecific gravity of liquids. In the next caſe the difference is ſtill leſs; for ÷ 90 . 0,0527 :: 56 . 0,0327; but 56° of heat produced in reality a dilatation of 0,0348 for 1,4650 - 1,4302 = 0,0348, ſo that the calculation is deficient only in $\frac{21}{10000}$.

I afterwards tried another, and ſomewhat ſtronger, ſpirit of nitre, whoſe ſpecific gravity was

	Deg.		Sp. gravity.
at	34	-	1,4750
	49	-	1,4653
	150	-	1,3792

Here alſo the expanſions are nearly proportional to the degrees of heat; for 116° of heat (the difference between 34 and 150) produce an expanſion of 0,0958; and 15° of heat (the difference between 34 and 49) produce an expanſion of 0,0097, and by calculation 0,0123, which laſt differs from the truth only by $\frac{26}{10000}$.

By this experiment we ſee, that the ſtronger the ſpirit of nitre is, the more it is expanded by the ſame degree of heat: for if the ſpirit of nitre of the laſt experiment were expanded in the ſame proportion as in the firſt, its dilatation by 116° of heat ſhould be 0,0679, whereas it was found to be 0,0958.

As the dilatation of ſpirit of nitre is far greater than that of water by the ſame degree of heat, and as it conſiſts only of acid and water, it clearly follows, that its ſuperior dilatability muſt be owing to the acid part; and hence, the more acid is contained in a given quantity of ſpirit of nitre, the greater is its dilatability. We might therefore ſuppoſe, that the dilatation of ſpirit of nitre was intermediate betwixt that of the quantity of water it contains and that of its quantity of acid; but there exiſts another power alſo which prevents this ſimple reſult, namely, the mutual attraction of the acid and water to each other, which makes them occupy a leſs ſpace than the ſum of their joint volumes, which condenſation I have therefore called their *accrued denſity*. Taking this into the account, we may conſider *the dilatation of ſpirit of nitre as equal to thoſe of the quantities of water and acid it contains minus the condenſation they acquire from their mutual attraction, and this rule holds as to all other heterogeneous compounds.*

To find the quanties of acid and water in ſpirit of nitre, whoſe ſpecific gravity was found in degrees of temperature different from thoſe for which the table was conſtructed, *viz.* 54, 55, or 56° of FAHRENHEIT, the ſureſt method is to find how much that ſpirit of nitre is expanded or condenſed by a greater or leſſer degree of heat, and then, by the rule of proportion, find what its denſity would be at 55°; but if this cannot be done, we ſhall approach pretty near the truth, if we allow $\frac{10}{1000}$ for every 15° of heat above or below 55° of FAHRENHEIT, when the ſpecific gravity of ſpirit is between 1,400 and 1,500; and $\frac{8}{1000}$ when the ſpecific gravity is between 1,400 and 1,300.

As to oil and ſpirit of vitriol I found the dilatations exceeding irregular, probably by reaſon of a white foreign matter, which is more or leſs ſuſpended or diſſolved in it, according to its greater or leſſer dilution. This matter I would not ſeparate, as I intended trying the denſity of this ſubſtance in the ſtate in which it is commonly uſed. In general I found, that 15° of heat cauſe a difference of about $\frac{6}{1000}$ in its ſpecific gravity when it exceeds 1,800; and of $\frac{3}{1000}$ when its ſpecific gravity is between 1, 400 and 1,300, its dilatation is greater than that of water, and ſo much greater as it is ſtronger.

The dilatations of ſpirit of ſalt are very nearly proportional to the degrees of heat, as appears by the following table.

	Deg.		Sp. gravity.
at	33	-	1,1916
	54	-	1,1860
	66	-	1,1820
	128	-	1,1631

Hence $\frac{6}{1000}$ ſhould be added or ſubtracted for every 21° above or below 55° in order to reduce it to 55°, the degree for which its proportion of acid and water was calculated. The dilatability of this acid is much greater than that of water, and even than that of the nitrous acid of the ſame denſity.

I now proceed to examine the quantity of pure acids taken up at the point of ſaturation by the various ſubſtances they unite with.

OF THE MINERAL ALKALI.

That which I made uſe of was procured from Mr. TURNER, who by a peculiar and ingenious proceſs extracts it in the greateſt purity form common ſalt.

Of this alkali I rendered a portion tolerably cauſtic in the uſual manner, and evaporating 1 oz. of the cauſtic ſolution to perfect dryneſs, I found it to contain 20,25 gr. of ſolid matter. I was aſſured, that the watery part alone exhaled during the evaporation, as the quantity of fixed air contained in it was very ſmall, and to diſſipate this a much greater heat would be requiſite than that which I uſed. This dry alkali I immediately diſſolved in twice its weight of water, and ſaturating it with dilute vitriolic acid, found it to contain 2,25 gr. of fixed air, that being the weight which the ſaturated ſolution ſution wanted of being equal to the joint weights of the water, alkali, and ſpirit of vitriol employed.

The quantity of mere vitriolic acid neceſſary to ſaturate 100 gr. of pure mineral alkali I found to be 60 or 61 gr. the ſaturated ſolution, thus formed, being evaporated to perfect dryneſs weighed 36,5 gr.; but of this weight only 28,38 were alkali and acid, therefore the remainder, that is, 8,12 gr. were water. Hence, 100 gr. of GLAUBER's ſalt, perfectly dried, contain 29,12 of mere vitriolic acid, 48,6 of mere alkali, and 22,28 of water; but GLAUBER's ſalt cryſtallized contains a much larger proportion of water; for 100 gr. of theſe cryſtals being heated red-hot loſt 55 gr. of their weight. This loſs I ſuppoſe to ariſe merely from the evaporation of the watery part, and the remaining 45 contained alkali, water, and acid, in the ſame proportion as the 100 gr. of GLAUBER's ſalt, perfectly dried, abovementioned; then theſe 45 contained 13,19 gr. of vitriolic acid, 21,87 of fixed alkali, and 9,94 of water; conſequently 100 gr. of cryſtallized GLAUBER's ſalt contain 13,19 of vitriolic acid, 21,87 of alkali, and 64,94 of water.

I alſo ſaturated this alkali with the dephlogiſticated nitrous acid, and found that 100 gr. of the alkali took up 57 of the mere nitrous acid in the experiment I moſt depended on; but this quantity varied in ſome experiments a few grains, being ſometimes 60, and ſometimes 63 gr.; ſo that I conclude the proportion of this acid, taken up by the alkali, is nearly the ſame as that of the vitriolic acid. Suppoſing this quantity to be 57 gr. then 100 gr. Cubic nitre, perfectly dry, contain 30 of acid, 52,18 of alkali, and 17,82 of water; but Cubic nitre cryſtallized contains ſomething more water; for 100 gr. of theſe cryſtals loſe about 4 by gentle drying; therefore 100 gr.

of the cryſtallized ſalt contain 28,8 of acid, 50,09 of alkali, and 21,11 of water.

Of mere marine acid, 100 gr. of this alkali required from 63 to 66 or 67 gr.; perhaps one reaſon of this variety is, that it is exceeding hard to hit the true point of ſaturation. Allowing it to be 66 gr. then 100 gr. of perfectly dry *common ſalt* contain nearly 35 of real acid, 53 of alkali, and 13 of water; but 100 gr. of the cryſtallized ſalt loſe 5 by evaporation; then 100 gr. of theſe cryſtals contain 33,3 of acid, 50 of alkali, and 16,7 of water.

The proportion of fixed air, alkali, and water, in cryſtallized mineral alkali, I inveſtigated thus: 200 gr. of theſe cryſtals were diſſolved in 240 of water; the ſolution was ſaturated by ſuch a quantity of ſpirit of nitre as contained 40 of mere nitrous acid; hence I inferred, that theſe 200 gr. of alkali contained 70 of real alkali. The ſaturate ſolution weighed 40 gr. leſs than the ſum of its original weight, and that of the ſpirit of nitre added to it; therefore it loſt 40 gr. of fixed air. The remainder, therefore, of the original weight of the cryſtals muſt have been water, that is, 90 gr.; conſequently 100 gr. of theſe cryſtals contained 35 of alkali, 20 of fixed air, and 45 of water.

This proportion is, particularly with regard to the alkali, very different from that found by Mr. BERGMAN and LAVOISIER, which I impute to their having uſed ſoda recently cryſtallized. Mine had been made ſome months, and probably loſt much water and fixed air by evaporation, which altered the proportion of the whole. According to the calculation of theſe philoſophers 100 gr. of this alkali takes up 80 of fixed air.

The ſpecific gravity of the cryſtallized mineral alkali weighed in æther I found to be 1,421.

OF THE VOLATILE ALKALI.

It is not poſſible by the old chymical methods to find the proportion of the ingredients in volatile alkalies, whether in a liquid or in a concrete ſtate; ſeeing that, though it may be ſeparated from fixed air, yet it cannot from water, on account of its extreme volatility. Then to find this proportion we muſt recur to the experiments of Dr. PRIESTLEY, who by his new analyſis produced this alkali free from the aërial acid and water in the form of air: and in the third volume of his Obſervations, p. 294. informs us, that $1\frac{6}{7}$ meaſures of alkaline air take up, and are ſaturated by, 1 meaſure of fixed air. Let us ſuppoſe the meaſure to contain 100 cubic inches; then 185 cubic inches of alkaline air take up 100 of fixed air; but 185 cubic inches of alkaline air weigh, at a medium, 42,55 gr.; and 100 cubic inches of fixed air weigh 57 gr.; then 100 gr. of pure volatile alkali, free from water, take up 134 of fixed air.

On expelling its aërial acid from a parcel of this alkali in a concrete ſtate, and formed by ſublimation, I found 100 gr. of it to contain 53 of fixed air, and therefore, according to the preceding reaſoning, 39,47 of real alkali and 7,53 of water *per cent.*

Saturating a ſolution of this alkali with the vitriolic, nitrous, and marine acids, I found, that 100 gr. of the mere alkali take up 106 of mere vitriolic acid, 115 of the nitrous, and 90 of the marine.

The ſpecific gravity of the concrete volatile alkali weighed in æther was 1,4076.

The proportion of water in the different ammoniacal ſalts I have not been able to find, on account of their volatility; but believe it to be very ſmall, as volatile alkali and fixed air cryſtallize without the help of water, when both are in an aërial ſtate.

OF CALCAREOUS EARTH.

I firſt diſſolved this earth in the nitrous acid, and found that, after allowing for the loſs of fixed air and the quantity of water I formerly mentioned, 100 gr. of the pure earth take up 104 of mere nitrous acid. Inſtead of diſſolving this earth immediately in the vitriolic acid, I precipitated its ſolution in the nitrous by the gradual addition of the vitriolic, and found that to effect this 91 or 92 gr. only of mere vitriolic acid were required.

100 gr. of this pure earth demand for their ſolution 112 of mere marine acid. The ſolution, which is at firſt colourleſs, grows greeniſh on ſtanding. Natural Gypſum varies in its proportion of acid, earth, and water, 100 gr. of it containing from 32 to 34 of acid, and alſo of earth, and from 26 to 32 of water. The artificial contains 32 of earth, 29,44 of acid, and 38,56 of water; when well dried it loſes about 24 of water, and therefore contains 42 of earth, 39 of acid, and 19 of water *per cent.*

100 gr. *nitrous ſelenite*, carefully dried, contain 33,28 of acid, 32 of earth, and 34,72 of water.

100 gr. *marine ſelenite*, well dried, ſo as to loſe no part of the acid, contain 42,56 of acid, 38 of earth, and 19,44 of water.

OF

OF MAGNESIA OR MURIATIC EARTH.

This earth, perfectly dry and free from fixed air, could not be diſſolved in any of the acids without heat. In the temperature of the atmoſphere even the ſtrongeſt nitrous acid did not act upon it in twenty-four hours; but in a heat of 180° theſe acids diluted with four or ſix times their quantity of water attacked it very ſenſibly; but as much of the acids is diſſipated by heat, I could not judge of the exact quantity of acid requiſite to diſſolve a given quantity of it, any otherwiſe than by precipitating the ſolutions by another ſubſtance, whoſe capacity for taking up acids was known. The ſubſtance I uſed was a tolerably cauſtic vegetable alkali. By this method I found, that 100 gr. of pure magneſia take up 125 gr. of mere vitriolic acid, 132 of the nitrous, and 140 of the marine. None of theſe ſolutions reddened vegetable blues; all of them appeared to contain ſomething gelatinous; that in the marine acid became greeniſh on ſtanding for ſome time.

100 gr. of perfectly dry Epſom ſalt contain 45,67 of mere vitriolic acid, 36,54 of pure earth, and 17,83 of water; but 100 gr. of cryſtallized Epſom loſe 48 by drying, and conſequently contain 23,75 of acid, 19 of earth, and 57,25 of water. Common Epſom ſalt contains an exceſs of acid, for its ſolution reddens vegetable blues.

100 gr. of nitrous Epſom, well dried, contain 35,64 of acid, 27 of pure earth, and 37,36 of water.

The ſolution of marine Epſom cannot be tolerably dried without loſing much of its acid, together with the water.

The ſpecific gravity of pure muriatic earth is 2,3296.

OF EARTH OF ALLUM OR ARGILLACEOUS EARTH.

This earth I found to contain about 26 *per cent.* of fixed air, though I had previouſly kept it red-hot for half an hour: this ſurpriſed me much, as moſt writers ſay it contains ſcarce any. It diſſolved in acids with a moderate efferveſcence until the heat was raiſed to 220°, after which I found the ſolution lighter than the quantities employed in the proportion I mentioned.

100 gr. of this earth (excluſive of the fixed air) require 133 of the *mere* vitriolic acid to diſſolve them. This ſolution I made in a very dilute ſpirit of vitriol, whoſe ſpecific gravity was 1,093, in which the proportion of acid to that of water was nearly as 1 to 14. This ſolution contained a ſlight exceſs of acid, turning vegetable blues into a browniſh red; but it cryſtallized when cold, and the cryſtals were of the form of allum; ſo that I believe this to be nearly the proper proportion of its acid and earth; but there was not water enough to form large cryſtals. As this ſolution contained an exceſs of acid, I added more earth to it, but could not prevent its tinging blue paper red, until it formed an inſoluble ſalt, that is, one that required an exceeding large quantity of water to diſſolve it, and while part was thus become inſoluble, yet another part would ſtill retain an exceſs of acid; ſo that at the ſame time part would be ſuperſaturated with earth, and another with acid, if tinging vegetable blues be a mark of an exceſs of acidity, which indeed in this caſe ſeems dubious.

100 gr. of Allum, perfectly dried, contain 42,74 of acid, 32,14 of earth, and 25,02 of water; but cryſtallized allum loſes 44 *per cent* by deſiccation; therefore 100 gr. of it contain 23,94 acid, 18 of earth, and 58,06 of water.

100 gr. of this pure earth take up as far as I can judge 153 of the mere nitrous acid. The ſolution ſtill reddened vegetable blues; but after the addition of this quantity of pure earth, I think it was, that an inſoluble ſalt came to be formed. The ſolution, when cold, grew turbid, and could not be wholly diſſolved by 500 times its weight of water.

The ſame quantity of pure earth requires 173,45 of the mere marine acid for its ſolution; but the ſolution ſtill reddens vegetable blues. After this an inſoluble ſalt was formed; but the beginning of its formation is difficultly diſcovered both in this and in the former caſes.

The ſpecific gravity of argillaceous earth, containing 25 *per cent.* of fixed air, I found to be 1,9901.

OF PHLOGISTON.

Before I proceed to inveſtigate its proportion in various compounds, and particularly in phlogiſticated acids, it will be neceſſary to ſay ſomething of its nature.

It is allowed on all hands, that fixed air, or the Aërial Acid, as it is more properly called, is capable of exiſting in two ſtates; the one fixed, concrete, and unelaſtic, as when it is actually combined with calcareous earth, alkalies, or magneſia; the other, fluid, elaſtic, and aëriform, as when it is actually diſengaged from all combination. In its concrete and unelaſtic ſtate it can never be produced ſingle and diſengaged from other ſubſtances; for the moment it is ſeparated from them, it aſſumes its aërial and elaſtic form. The ſame thing may be ſaid of phlogiſton: it can never be produced in a *concrete ſtate*, ſingle and uncombined with other ſubſtances; for the inſtant it is diſengaged from them, it appears in a fluid and elaſtic form, and is then commonly called *inflammable air.* Theſe different ſtates of the ſame ſubſtance ariſe, according to the immortal diſcoveries of Dr. BLACK, from the different portions of elementary fire contained in ſuch ſubſtance, and abſorbed by it, whilſt its ſenſible heat remains the ſame, and hence called its *ſpecific fire.* For want of attention to theſe different ſtates, the very exiſtence of phlogiſton as a diſtinct principle has been frequently called in queſtion, and chemiſts have been required to exhibit it ſeparate in its fixed ſtate, without recollecting, that neither can fixed air be ſhewn ſeparate in a concrete ſtate, nor that phlogiſton may alſo be in the ſame predicament; while others have totally miſtaken the nature of inflammable air, and imagined it to be a combination of acid and phlogiſton. The reaſon why fixed air cannot be ſeparated from any ſubſtance in a concrete ſtate is, becauſe when it is ſeparated, for inſtance by means of an acid, there is always a double decompoſition, the acid yielding its ſpecific quantity of fire to the concrete fixed air, which then aſſumes an aërial form, while the fixed air yields the ſubſtance it was combined with to the acid. This is ſo true, that though a ſolution of lime in the nitrous acid yields a conſiderable quantity of heat, yet a ſolution of chalk in that acid ſcarcely yields any; for all the fire that is ſet looſe, and rendered ſenſible in the firſt caſe, is abſorbed by the fixed air in the ſecond caſe, being preciſely that which converts it into an aërial form. The ſeparation of phlogiſton from a metallic earth in the form of inflammable air ariſes from the ſame cauſe, the diſſolving acid yielding its fire to the phlogiſton, which then aſſumes an aërial form, while the phlogiſton yields the metallic earth to the acid. It is true, that much ſenſible heat is produced on this occaſion, for which three ſubſtantial reaſons may be aſſigned; firſt, the proportion of fixed air in a given weight of crude calcareous earth, is much greater than that of phlogiſton in any metal, as will hereafter be ſhewn, it being in the former one-third of the whole, and that of phlogiſton in the latter for the moſt part not even one-ſixth. Secondly, much of the phlogiſton combines with the acid itſelf during the ſolution, and expels part of its ſpecific quantity of fire, as Dr. CRAWFORD has ſhewn, and as I have ſince experienced; and this fire muſt occaſion ſenſible heat. Thirdly, much of the phlogiſton, during ſolution, unites to the ſurrounding atmoſphere, expelling alſo part of its ſpecific fire, and this alſo muſt occaſion ſenſible heat; and hence it is, that metallic ſolutions *in vacuo* are generally attended with leſs heat, though with a more violent efferveſcence than in open air. The ſolution of metallic calces is not attended with as much heat as that of their reſpective metals, not only becauſe neither the diſſolving acids nor the ſurrounding air is much phlogiſticated; but alſo becauſe they contain an elaſtic fluid in a concrete ſtate, which abſorbs much of the fire given out by the diſſolving acids, as it acquires an aërial ſtate.

The origin and formation of inflammable air being thus explained, I now proceed to ſhew its identity and homogeneity with phlogiſton. By phlogiſton is generally underſtood that principle in combuſtible bodies on which their inflammability principally depends; that principle to which metals owe their malleability and ſplendor; that which combined with vitriolic acid forms ſulphur; that which diminiſhes reſpirable air. Now inflammable air is that very principle which alone is truly inflammable, as Mr. VOLTA has elegantly ſhewn. In effect, combuſtible ſubſtances are either animal or vegetable, as horns, hair, greaſe, wood, &c.; from all of which Dr. HALES has extracted inflammable air; or charcoal, from which Mr. FONTANA has extracted it, as did Dr. PRIESTLEY from reſins, ſpirit of wine, and æther, in all which it is the only principle that is inflammable, and they are inflammable only in proportion as they yield it; or phoſphorus, from whoſe acid Dr. PRIESTLEY has obtained this air by means of minium, for it was the acid, and not the minium, that contained it, as Dr. PRIESTLEY rightly conjectured, the acid obtained by deliqueſcence being never thoroughly dephlogiſticated until heated and vitrified, as Mr. MARGRAAF has ſhewn; or they are mineral ſubſtances, as ſulphur, from which inflammable air has been ſeparated by means of fixed alkalies, and, according to Dr. PRIESTLEY, alſo by means of marine air, or bitumens or bituminous ſubſtances, all of which may be made to yield it; or metallic ſubſtances, as zinc and regulus of arſenic, both of which are inflammable; but neither of them is ſo when deprived of its inflammable air: this is, therefore, the true and only principle of inflammability in any ſubſtance. I acknowledge that the inflammable air, proceeding from almoſt all theſe ſubſtances, is exceeding impure; that it contains from ſome a mixture of aërial acid or of oil, and from all ſome part of the ſubſtance which yields it or expels it, and hence its ſmell is different, according to the claſs of the ſubſtances from which it is extracted; but it is equally true, that none of theſe ſubſtances contribute to its inflammability; on the contrary, it is ſo much the leſs inflammable (that is, requires ſo much more air to be mixed with it before it flames) as it contains more of theſe heterogeneous ſubſtances. Hence inflammable air of the moraſſes is never totally conſumed*; and, on the contrary, inflammable air, from metals which is the pureſt of all, is alſo the moſt inflammable.

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* 15 Roz. 146.

Secondly, Inflammable air is alſo the principle which reduces metallic earths to a metallic ſtate, and gives them their metallic ſplendor. This has been proved analytically and ſynthetically, and therefore may be ſaid to be as completely demonſtrated as any thing in natural philoſophy: thus Dr. PRIESTLEY has extracted inflammable air from iron and zinc by heat alone; and the iron, thus ſtripped of its phlogiſton, loſt its ſplendor, and was of a black colour, which is that which iron, ſlightly dephlogiſticated, always aſſumes, as appears by *martial æthiops:* ſo alſo zinc and regulus of arſenic, when once inflamed, loſe their metallic appearance: ſo alſo a mixture of lead and tin inflames in a moderate heat, and then both are converted into a calx deſtitute of ſplendor and malleability. On the other hand, if a current of inflammable air, in the act of combuſtion, be directed on the calces of iron, lead, or mercury, they are immediately revived and reſtored to their metallic form, as appears by the experiment of Mr. CHAUSSIER*. The following experiment is ſtill more concluſive: if a poliſhed plate of iron be put into a ſaturate and dilute ſolution of copper in the vitriolic or marine acids (I mention theſe becauſe they are commonly uſed for the production of inflammable air, though the reſult is the ſame when other acids are uſed), no efferveſcence will ariſe, no inflammable air will be caught; but the iron will be diſſolved, and the copper precipitated in its metallic form. Here inflammable air muſt be produced as uſual, for the acid quits the copper and diſſolves the iron; but this inflammable air inſtantly loſes its aërial form, and unites to the copper, juſt as fixed air leaves alkalies to unite to lime without any efferveſcence; and by this ſame inflammable air is the copper evidently reduced, acquiring ſplendor, malleability, and every other metallic property. But if the ſolution of copper be not ſaturated with copper, a ſmall quantity of inflammable air may be caught, as the exceſs of acid will diſengage more of it from the iron than the calx of copper can take up. Inflammable air is then the principle that metallizes metallic earth; and if metals contain only a ſpecific earth and phlogiſton, inflammable air certainly contains nothing elſe but phlogiſton. If iron and the arſenical acid be digeſted together, no inflammable air is produced; but the arſenical acid is, in great meaſure, converted into white arſenic, as Mr. BERGMAN has obſerved, and alſo Mr. SCHEELE*; what reaſon can be aſſigned why inflammable air is not produced by this as well as by all other acids; but that this metallic acid received it, and was by it reduced to a ſemi-metallic form, as by pure phlogiſton? Yet this acid produces inflammable air, from zinc becauſe zinc gives out more phlogiſton than the regulus of arſenic can take up; but it attracts and is metallized by a part of it, and it is only the exceſs that appears in the form of inflammable air, as Mr. SCHEELE has remarked. This inflammable air, indeed, is not pure, for it holds ſome of the regulus in ſolution; but this portion of regulus does not enter into its compoſition, as is very evident.

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* 10 Roz. 313.

Thirdly, Inflammable air is the ſubſtance which, with vitriolic acid, forms ſulphur, for it is the very ſubſtance which the vitriolic acid ſeparates from metals; and this ſubſtance, ſo ſeparated, when in ſufficient quantity, and in proper circumſtances, unites to it in ſuch proportion as to form common ſulphur. Thus ſulphur is formed by diſtilling concentrated vitriolic acid with iron or biſmuth, or by diſtilling tartar vitriolate with regulus of antimony. It is this alſo that diminiſhes reſpirable air, as Dr. PRIESTLEY has clearly ſhewn in the 5th vol. of his Obſervations, p. 84.; for though in its complete aërial ſtate, after it has abſorbed that large quantity of fire requiſite to its aërial form, it difficultly and ſlowly unites to reſpirable air in the heat of the atmoſphere, their points of contact through their difference of denſity being very ſmall, and there being no ſubſtance at hand to receive the large portion of elementary fire they both contain, and of which they muſt loſe a large proportion before they can combine together; yet while inflammable air is (as Dr. PRIESTLEY elegantly expreſſes it) in its *naſcent* ſtate, before it acquires its whole quantity of ſpecific fire, reſpirable air eaſily unites to it, and is diminiſhed in proportion to its purity; but if to a mixture of both, igneous particles of ſufficient denſity to be viſible be introduced, a degree of heat is excited, which, as it rarifies the dephlogiſticated part of reſpirable air to a greater degree than it can inflammable air *, brings both into nearer contact, increaſes their attraction to each other, and both uniting give out their fire, or in other words *inflame*, when in proper proportion to each other, without any decompoſition of either, unleſs the loſs of a great part of their ſpecific fire be called a decompoſition, which loſs is not uſually called a decompoſition; for water is never ſaid to be decompoſed when it becomes ice, nor metals when they become ſolid on cooling.

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* 2 Nov. Act. Upſal. p. 210. Kon. Veten. Accad. Handlingar, vol. 36. p. 288.

In anſwer to all this it will be ſaid, that inflammable air undoubtedly contains phlogiſton, which produces all the beforementioned effects; but that the phlogiſton it contains is united to ſome other ſubſtance, which ſome will have to be an acid, ſome an earth, and others reſpirable air. To theſe hypotheſes I ſhall oppoſe one general obſervation, which is, that ſince inflammable air, when pure, that is, when diſengaged from all heterogeneous ſubſtances which no way contribute to its inflammability, has always the ſame properties; it muſt, if it conſiſts of phlogiſton combined with any other ſubſtance, be always united to the ſame ſpecific ſubſtance; that is, if this be an acid, it muſt be always the ſame ſpecies of acid, or if an earth, it muſt be always the ſame ſpecies of earth; for we find, that ſubſtances, which are only *generically* the ſame, always produce, with any other given ſubſtance, compounds whoſe properties are very different from each other. Thus we ſee that the different ſpecies of alkalies, or earths, or metals, produce with one and the ſame ſpecies of acid compounds eſſentially different. This is a rule which, as far as I know, admits of no exception: and if we apply it to the abovementioned ſuppoſitions it will intirely deſtroy them; for it is impoſſible to think, that the phlogiſton can in every ſubſtance, that produces inflammable air, meet either the ſame acid, or earth, or any reſpirable air.

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* 5 PRIEST. 359.

But to be more particular, the following reaſons demonſtrate that an acid of any ſort cannot be the baſis of inflammable air. 1ſt. Inflammable air has been, by Dr. PRIESTLEY, ſeparated from metals by mere heat. Now metals contain no acid, except perhaps their dephlogiſticated calx, which thoſe eminent chemiſts, BERGMAN and SCHEELE*, ſuſpect to be of an acid nature; but theſe calces cannot enter into the compoſition of inflammable air, otherwiſe the inflammable air of each different metal would have different properties, as already ſhewn: nor indeed are theſe the acids that have been ſuppoſed to enter into the compoſition of inflammable air; but rather thoſe acids by whoſe means it is extricated. But as this air is extricated from metals, not only by acids, but alſo by alkalies *, this ſuppoſition muſt vaniſh of courſe.

The ſame reaſons militate with equal ſtrength againſt the ſuppoſition that an earth of any kind enters into the compoſition of this air; nor is there an inſtance of any earth rendered permanently fluid by any means, except in ſparry air. Beſides, if it were a metallic earth, it muſt neceſſarily be ſuppoſed to be in a metallic ſtate; and how then could it eſcape the action of all kind of acids? for no acid is capable of decompounding inflammable air. Laſtly, reſpirable air cannot be ſaid to be the baſis of inflammable air, unleſs we ſuppoſe that reſpirable air enters into the compoſition of metals; for Dr. PRIESTLEY has, by ſolar heat, extracted inflammable air from them in a veſſel full of mercury, into which reſpirable air had no acceſs, and even *in vacuo*. Beſides, reſpirable air and phlogiſton form other compounds very different from inflammable air, *viz.* fixed and phlogiſticated airs as will preſently be ſeen.

It may alſo be fairly urged againſt all theſe ſuppoſitions, that they are not founded on any direct experiment, nor any known analogy, but merely gratuitous, or at leaſt deduced from experiments inadequate to their ſupport; whereas the opinion that inflammable air is nothing elſe than phlogiſton thrown into a fluid form by elementary fire, is directly founded on that experiment whereby inflammable air is ſeparated from metals by mere ſolar heat in the moſt perfect vacuum, juſt as fixed air united to marble and in a concrete ſtate (in which it is nearly of equal denſity with gold) is ſeparated from the marble, and thrown into a permanently fluid form by heat alone.

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* Mem. Par. 1776, p. 687.

In favour of the exiſtence of an acid in inflammable air, it has been ſaid, that if this air be paſſed through water tinged blue by litmus, it reddens inſtantly. I have ſeen this frequently happen when inflammable air has been extracted from iron by ſpirit of vitriol; but if this air be waſhed, by paſſing it through lime-water, and then paſſed through, or agitated in, an infuſion of litmus, it will not diſcolour it in the leaſt: this I have ſeen done by Mr. FONTANA in June 1779. It has alſo been ſaid, that inflammable air and alkaline air, mixed together, form a cloud; but this has been fully diſproved by Dr. PRIESTLEY, in the fourth volume of his Obſervations.

That an earth of any kind is eſſentially requiſite to the conſtitution of inflammable air, ſeems to me utterly improbable; nor do I know of any experiment from whence it can be inferred. That metallic ſubſtances may be held in ſolution by inflammable air is certain; but it is equally ſo, that they no way contribute to its inflammability *, and are quite diſtinct from it.

But the opinion, that inflammable air conſiſts of reſpirable air ſuper-ſaturated with phlogiſton, is grounded on very ſpecious arguments drawn from experiments to be found in various parts of Dr. PRIESTLEY's works, which deſerve ſo much the more attention as the facts mentioned by that excellent philoſopher are not to be queſtioned. I ſhall endeavour to ſtate them with accuracy; but ſhall at the ſame time accompany them with ſuch remarks as ſeem to me to invalidate the concluſion that has been drawn from them.

In the firſt volume of Dr. PRIESTLEY's Obſervations it appears, that a quantity of ſtrong inflammable air, having been agitated in a glaſs jar immerſed in a trough of water,

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* 2 PRIESTLEY, 268.

the ſurface of which was expoſed to the common atmoſphere, after the operation had continued ten minutes near one-fourth of the quantity had diſappeared; the remainder became fit for reſpiration, and yet was weakly inflammable. By further agitation it was diminiſhed half, and then admitted a candle to burn in it though feebly; but, on continuing the agitation a little longer, it came to extinguiſh a candle. Upon this I ſhall remark, firſt, that it clearly follows, from this experiment, that if the external reſpirable air had no acceſs to the inſide of the jar, half nearly of the inflammable air was converted into, or conſiſted of, reſpirable air, ſince ſuch quantity of air was found in it after the operation. Now it is abſolutely impoſſible that either could happen; for inflammable air could not be converted into half nor even one-third or one-fourth of its volume of reſpirable air, as even one-fourth of reſpirable air contains more matter than four times its bulk of inflammable air; it is then evident, that the external air muſt have had acceſs to it. Secondly, I agitated about half a pint of inflammable air, obtained from iron and previouſly paſſed through lime-water and kept over mercury, in about twelve times its bulk of water, out of which its air had been boiled in a glaſs bottle cloſed with a glaſs-ſtopper. The agitation continued at ſeveral times at leaſt two hours. A large quantity of the air was indeed abſorbed, as appeared by opening the bottle in water; but the remainder appeared, by the nitrous teſt, as noxious, and was alſo found to be as inflammable as at firſt. Even Dr. PRIESTLEY atteſts, that inflammable air, which had been united to water for one month, was afterwards as inflammable as ever. 3 PR. 267.

The true explanation of the firſt experiment appears, therefore, to be the following: Firſt, Water eaſily imbibes inflammable air, but does not combine with it; for after it has imbibed one-fourteenth of it, its taſte is no way altered, as Dr. PRIESTLEY has obſerved. 1 PR. 196. Water alſo eaſily imbibes common air: when, therefore, inflammable air is agitated in water having a communication with the atmoſphere, the inflammable air muſt neceſſarily be diminiſhed by reaſon of its abſorbtion, and the part ſo abſorbed immediately eſcapes out of the water into the atmoſphere, as is evident by the ſmell which is perceived when the quantity of inflammable air is conſiderable. This eſcape gives room for the further abſorbtion of the inflammable air which then eſcapes in the ſame manner. In the mean time the common air under the jar riſes into it, as appears by the direct experiments both of Dr. PRIESTLEY * and Mr. FONTANA; and hence the air in the jar muſt appear by the nitrous ſlightly phlogiſticated and reſpirable; but a further agitation will decompoſe the common air, as we ſhall ſoon ſee, and then a candle will be extinguiſhed. The ſame proceſs takes place when inflammable air ſtands long in water whoſe ſurface is expoſed to the atmoſphere.

Another experiment of the ſame tendency, but ſeemingly more deciſive, is to be found in the 4th vol. of Dr. PRIESTLEY's Obſervations, p. 368. There it is related, that a portion of inflammable air, incloſed in a glaſs tube, hermetically ſealed and heated until the glaſs was ſoftened, ſtained the glaſs black, and the tube being opened, the air was found reduced to one-third of its bulk; and this reſiduum was found to be mere *phlogiſticated air*, neither precipitating lime-water, nor being affected by nitrous air, or in the leaſt inflammable. Yet deciſive as this experiment appears, a little conſideration will ſhew the abſolute impoſſibility that inflammable air ſhould conſiſt of one-third phlogiſticated air and two-thirds phlogiſton;

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* 1 PR. 96. 159. 3 PR. 156. Phil. Tranſ. 1779, p. 443.

for, in the firſt place, one Cubic inch of phlogiſticated air weighs 0,377 of a grain: now let us ſuppoſe, that to this phlogiſticated air is added two-thirds of its bulk of phlogiſton; and to make the ſuppoſition ſtill ſtronger, let us alſo ſuppoſe, that phlogiſton has no weight; then, by the ſuppoſition, this compound of phlogiſticated air and phlogiſton will conſtitute inflammable air, and amount to a bulk of three Cubic inches, and theſe three Cubic inches will weigh no more than 0,377 of a grain; but if three Cubic inches of inflammable air weigh 0,377 of a grain, one Cubic inch ſhould weigh 0,105 of a grain, which cannot be; for then inflammable air would be little more than one-third lighter than common air, contrary to all the experiments that have been hitherto made, and particularly thoſe of Mr. CAVENDISH, FONTANA, and Dr. PRIESTLEY himſelf, which ſhew it to be about eleven times lighter than common air. Secondly, It is ſaid, that the matter which ſtained the glaſs black was the true phlogiſtic part of inflammable air, and was afterwards ſeparated by means of minium. This then contained no phlogiſticated air; but is it not certain, that if there had been enough of it, the minium would have been reduced and converted into lead? And might not inflammable air be again ſeparated from that lead, though no phlogiſticated or common air were at hand to ſupply its other ſuppoſed conſtituent part? Thirdly, In one of Dr. PRIESTLEY's experiments the inflammable air, contained in the glaſs tube which was moſt heated, was reduced to ſo ſmall a bubble that no experiment could be made on it: therefore, in this, at leaſt, the quantity of phlogiſticated air did not amount to one-third, but was quite inconſiderable; the remainder then being taken up by the calx of lead in the glaſs, was pure mere phlogiſton, ſo that this experiment is a ſtrong proof of my opinion.

Fourthly, If phlogiſton could be decompoſed by heat, and then leave a reſiduum of phlogiſticated air, amounting to one-third of its bulk, the diminution ariſing from its inflammation with common or dephlogiſticated air could never be ſo great as it is found to be by repeated experiments; for when inflammable and common air are fired in the proportion of eleven of the latter to four of the former, a bulk equal to the whole of the inflammable air, and to one-fifth of the common air, diſappears, according to Mr. VOLTA [*], and the diminution is about two-fifths of the whole, or more exactly out of fifteen meaſures, only 8,8 remain; but if the inflammable air were decompoſed, and one-third of it, being phlogiſticated air, ſhould remain, then not quite one-fifth of the whole would vaniſh, and the reſiduum ſhould be 10,54 meaſures. This evidently proves, that pure inflammable air is never decompoſed (unleſs the loſs of its fire be called a decompoſition); but in the act of inflammation is totally transferred upon the pure part of reſpirable air to which it unites. Fifthly, To obtain ſtill a clearer inſight into this matter, I intreated Mr. CAVALLO, who is very expert in the management of the blow-pipe, as well as in pneumatic experiments, to repeat this experiment in my laboratory. We accordingly filled a tube 10,5 inches long, and one-fourth of an inch in diameter, with inflammable air from iron received over mercury, and having made the tube red-hot throughout and black, and ſoftened it ſo far as to endanger the eſcape of the air, we opened it on mercury. The air was diminiſhed only one-tenth, and inflamed with an exploſion as loud as an equal quantity of the ſame inflammable air that had not been heated.

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[*] Roz. April 1779, p. 295.

The only queſtion that remains then is, whence the phlogiſticated air proceeded which Dr. PRIESTLEY mentions to have found? The circumſtance of his experiment would furniſh a plauſible anſwer; but the doctor has lately informed me, that he believes the air was really inflammable, but being a very ſmall quantity eſcaped before the flame could be applied.

It ſeems, therefore, ſufficiently proved, that inflammable air purified from the acids or other ſubſtances that expel it from its baſis, and alſo from all particles of the body to which it was originally united, ſuch as inflammable air from metals received on mercury, and well waſhed in lime-water, is one and the ſame ſubſtance with phlogiſton, differing only in quantity of fire, inflammable air containing nearly the ſame quantity of this element as the ſame bulk of atmoſpheric air, as Dr. CRAWFORD has found by ſome late experiments, an account of which will ſoon be laid before the public. This does not contradict that moſt important diſcovery of this ingenious philoſopher, that fire and phlogiſton repel each other: the meaning of this being only, that the addition of phlogiſton to any ſubſtance, as to reſpirable air, dephlogiſticated acids, metallic calces, expels part of the fire already contained in ſuch ſubſtance; and, on the contrary, by the removal of phlogiſton from any ſubſtance, the quantity of fire abſorbed by ſuch ſubſtance is increaſed.

It may appear extraordinary, ſuppoſing inflammable air and phlogiſton to be the ſame ſubſtance, that inflammable air ſhould mix ſo eaſily with water, whereas phlogiſton conſtantly repels and is repelled by it; but this intirely depends on the ſtate of this ſame ſubſtance, which, when fixed and concrete, is called *phlogiſton*, and, when rarified and aëriform, *inflammable air*. In this latter ſtate it mixes with water in proportion to its rarefaction,

faction, as it even does in the leſs denſe forms of its concrete ſtate: thus æther is totally abſorbed by ten times its weight of water. The animal oil of Dippel mixes intirely with water; ſo does pure Petrol, and eſſential oils frequently diſtilled, and the ſpiritus rector of plants.

Much more remains to be ſaid of the different ſtates of phlogiſton from its moſt rarefied known ſtate, *viz.* that of inflammable air, to its moſt condenſed ſtate, that in which it is combined with metallic earths, &c. I have already diſtinguiſhed eight intermediate ſtates each differing from the other by the portion of elementary fire they contain, this quantity being, as far I can judge directly, as the rarefaction of the phlogiſton; but theſe reſearches are foreign to my preſent ſubject. I ſhall only remark, that phlogiſton, in a ſtate perhaps 100 times rarer than inflammable air, and conſequently containing much more fire, may poſſibly conſtitute the electric fluid.

P. S. Since I wrote the above, I have been honoured with a letter from Dr. PRIESTLEY, in which he informs me, that he has reduced the calces of iron, copper, lead, and tin, merely by melting them in inflammable air by means of a burning glaſs. A certain quantity of inflammable air was abſorbed by each during their reduction; but the unabſorbed part was equally inflammable, ſo that there was no decompoſition; but the remainder was of the ſame nature as the part abſorbed. He alſo, by the ſame means, converted nitrous vapour into nitrous air, and the phoſphoric acid into phoſphorus. And ſince the communication of the laſt mentioned experiments, which ſeem to him alſo a direct proof of the identity of inflammable air and phlogiſton, he has been ſo obliging as to inform me, that he has revived the calces of metals in *alkaline air* as well as ſin *inflammable air*, and alſo formed a phoſphorus; and that he has little doubt but that he ſhall be able to produce any thing elſe in which phlogiſton is ſuppoſed to be concerned. This, he ſays, agrees with ſeveral of his former experiments, eſpecially that in which he produces inflammable air from alkaline air, by means of the electric ſpark and volatile alkali from iron, ſuperſaturated with phlogiſton by means of nitrous air, which he has repeatedly done ſince the publication of his laſt volume. This obſervation, he adds, may help to explain ſome things in the theory of chemiſtry, eſpecially the affinity which all acids have both with *phlogiſton* and with *alkalies*; but, he ſays, that alkaline air contains ſomething elſe beſides phlogiſton; becauſe when this air is uſed, there is always a reſiduum of ſomething that is *neither alkaline nor inflammable air*; but he wants more ſunſhine to complete and extend his experiments on this ſubject *.

OF THE QUANTITY OF PHLOGISTON IN NITROUS AIR.

100 gr. of filings of iron being diſſolved in a ſufficient quantity of very dilute vitriolic acid produced, with the aſſiſtance of heat gradually applied, 155 cubic inches of inflammable air, the barometer at 29,5, and the thermometer between 50 and 60°. Now inflammable air and phlogiſton being the ſame thing, this quantity of inflammable air amounts to 5,42 gr. of phlogiſton.

Again, 100 gr. of iron, diſſolved in dephlogiſticated nitrous acid, in a heat gradually applied and raiſed to the utmoſt, afford 83,87 cubic inches of nitrous air. And as this nitrous air contains nearly the whole quantity of phlogiſton which iron will part with (it being more completely dephlogiſticated by this acid than by any other means). it follows, that 83,87 cubic inches of nitrous air contain at leaſt 5,42 gr. of phlogiſton; but it may reaſonably be thought that the whole quantity of phlogiſton which iron will part with is not expelled by the vitriolic acid, and that nitrous acid may expel and take up more of it. To try whether this was really ſo, I calcined a certain quantity of green vitriol, until its ferruginous baſis was quite inſipid; I then extracted from 64 gr. of this ochre two cubic inches of nitrous air, conſequently 100 gr. of this ochre would give 3,12 cubic inches of nitrous air; and if 83,87 cubic inches of nitrous air contain 5,42 of phlogiſton, then 3,12 cubic inches of this air contain 0,2 of a grain of phlogiſton; conſequently, nitrous acid extracts from 100 gr. of iron two tenths of a grain more phlogiſton than the vitriolic acid does; therefore 83,87 cubic inches of nitrous air, containing nearly all the phlogiſton which iron gives out, contain 5,62 gr. of phlogiſton.

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* Since this paper was committed to the preſs, I find that Mr. PELLETIER has reduced the arſenical acid to a regulus, by merely paſſing inflammable air through the ſolution of that acid in twice its weight of water. ROZ. Journ. February 1782.

Then 100 *cubic inches of nitrous air contain* 6,7 *gr. of phlogiſton*, and ſince 100 cubic inches of nitrous air weigh 39,9 gr. they muſt alſo contain 33,2 *gr. of nitrous acid.*

Alſo, 100 *gr. of nitrous air contain* 16,792 *of phlogiſton, and* 83,208 *of acid.*

When firſt I made theſe experiments I imagined, that the nitrous air thus expelled contained all the phlogiſton of the metals diſſolved in the nitrous acid; as this acid is well known to dephlogiſticate metals as perfectly as poſſible; but I ſoon obſerved, as did Dr. PRIESTLEY and Mr. FONTANA, that the greater part of this is air reſorbed and detained in the ſolution, the acid and calx having, according to the beautiful remark of Mr. SCHEELE, a greater attraction to phlogiſton than either ſeparately; yet that the calculation is nearly juſt, will appear clearly in my next paper, by its coincidence with the quantity of phlogiſton diſcovered in lead by Dr. PRIESTLEY and that which is contained very evidently in regulus of arſenic, ſilver, and quickſilver.

OF THE QUANTITY OF PHLOGISTON IN FIXED AIR.

Before I attempt to determine this quantity, it will be neceſſary to prove that it contains any; and for this purpoſe minutely to examine its nature and origin.

Dr. PRIESTLEY firſt diſcovered that in all proceſſes, wherein phlogiſton is diſengaged from any ſubſtance, as in *combuſtion, reſpiration, calcination of metals, putrefaction, decompoſition of nitrous air by reſpirable air*, &c. fixed air is precipitated from the common or dephlogiſticated air in which theſe proceſſes are performed, and that theſe laſt airs, are diminiſhed both in weight and bulk, and are afterwards leſs fit, or abſolutely unfit, for theſe proceſſes, according to the quantity of phlogiſton that was ſet looſe. Theſe facts are admitted by all, let their ſyſtems be what they may. However, Dr. PRIESTLEY thinks he has ſeen one exception to this general rule; for, he ſays, that in the combuſtion of inflammable and common air no fixed air is precipitated, 5 PR. 124. He alſo ſeems inclined to admit another exception in the caſe of the combuſtion of ſulphur.

The queſtions that here ariſe are, firſt, whether the fixed air that appears in theſe circumſtances proceeded from the reſpirable air or not? Secondly, If it proceeded from the reſpirable air, whether it pre-exiſted in that air; or whether it was generated during the proceſs that exhibits it? and if ſo, what are its conſtituent parts?

The firſt queſtion is eaſily anſwered; for in ſuch phlogiſtic proceſſes as are attended with the deſtruction of the ſubſtances that are known to contain fixed air, as thoſe of the animal and vegetable kingdom, the fixed air may be ſuppoſed to proceed in many caſes, both from the decompoſed ſubſtance and from the reſpirable air; and of this ſort are the proceſſes of combuſtion of moſt animal and vegetable ſubſtances, and fermentation; but the fixed air, that appears in ſuch phlogiſtic proceſſes as are performed on ſubſtances that contain no fixed air, muſt be deemed to proceed from the reſpirable air ſingly. And of this caſe we have four clear inſtances; the calcination of metals; the decompoſition of nitrous air by reſpirable air; the diminution of common air by the electric ſpark; and, laſtly, its diminution by amalgamation.

And firſt as to the calcination of metals, Dr. PRIESTLEY has obſerved, that by this operation reſpirable air (and only reſpirable air) is diminiſhed between one-fourth and one-fifth, both in its weight and bulk; but Mr. LAVOISIER has demonſtrated, that nothing is loſt or eſcapes through the veſſels (as Mr. SCHEELE would have it); for the weight and materials continue undiminiſhed when the operation is performed in cloſe veſſels*. That part, therefore, which the air loſes is taken up by the metallic calx, which accordingly is found to gain the very weight which the air loſes. Now the air contained in the calx is fixed air; for Mr. LAVOISIER alſo obſerved, that by the calcination of lead, by ſolar heat, over lime-water, the water was rendered ſlightly turbid†. It is true, that Dr. PRIESTLEY, in a ſimilar experiment, did not obſerve this turbidity; but he accounts for this circumſtance very juſtly, by ſuppoſing, that the calx of lead abſorbed the fixed air preferably to the lime. And this ſuppoſition is not gratuitous; for metallic calces, and particularly thoſe of lead, are known to attract fixed air as ſtrongly as quick lime, or rather more ſtrongly*: and what ſets this matter beyond all doubt, the calces of lead all yield fixed air by heat, and the grey calx of lead, in particular, which was that produced by Dr. PRIESTLEY, in the experiment to which I allude, affords by heat fixed air only. Other calces of lead after fixed air afford alſo dephlogiſticated air; but this I ſhall ſhew alſo to have been originally fixed air. If filings of iron be mixed with water in cloſe veſſels, they will be converted into ruſt, and the incumbent air diminiſhed one-fourth, as Mr. LAVOISIER atteſts †; but Dr. PRIESTLEY has ſhewn, that ruſt of iron yields ſcarce any other than fixed air, which may be expelled out of it by mere heat ‡. Nay, iron alone, expoſed to common air over a veſſel of water for three months, reduced this air one-fifth; and being expoſed to dephlogiſticated air, over a veſſel of mercury, it reduced it one-tenth in nine months §. In all theſe caſes the fixed air could ſurely come from nothing elſe but the incumbent reſpirable air and the phlogiſton of the metal.

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* Mem. Par. 1774.

† 1 LAVOIS. 291.

Secondly, It is well known, that if nitrous air be decompoſed by reſpirable air over lime-water, the lime will be precipitated ‖. In this caſe alſo, the fixed air muſt proceed from the reſpirable air and the phlogiſton of the nitrous air; for it cannot proceed from the nitrous acid, as this acid is not decompoſed, but is taken up by the water over which the mixture of both airs is made, as Mr. BEWLY has undeniably proved: and hence it is, that unleſs a large quantity of lime-water be uſed ſo as to contain enough for both the nitrous and aërial acids to act on, there will be no precipitation of lime, as Mr. FONTANA has obſerved; for the nitrous acid will ſeize on the lime preferably to the aërial. Dr. PRIESTLEY indeed obſerved, that if a bladder, filled with nitrous air, be dipped in lime-water, it occaſions a precipitation of lime on the ſurface of the water. 1 PR. 213. But he elſewhere acknowledges, that this proceeds from the inability of the bladder to confine nitrous air. 1 PR. 76. and 128, which Mr. BAUME alſo long ago obſerved, without knowing any thing more of this air. BAUME *ſur l'Æther*, 285. The phlogiſton paſſes through the bladder, and unites to the common air contiguous to it*. Beſides, nitrous air acts on the bladder itſelf, and extracts fixed air from it. 1 PR. 214. Hence alſo, if rain-water carefully boiled, and freed from its own air, be made to abſorb a quantity of nitrous air, it will again, on boiling, yield it back as pure as at firſt; but if common water be made to imbibe nitrous air in the ſame manner, it will, on boiling, yield alſo a portion of fixed air. 3 PR. 109. Does not this happen clearly becauſe common water contains atmoſpheric air, or air ſomewhat purer, which is converted into fixed air by mixture with the nitrous air? This experiment alſo ſhews, that water itſelf never unites to phlogiſton, ſince it does not take any from nitrous air, where the union of phlogiſton to the acid is of the laxeſt kind.

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* VOGEL, § 599. 2 N. Act. Upſal. 240. IX Mem. Scav. Etrang. 544.

† 1 LAVOIS. 192.

‡ 2 PR. 212.

§ 2 PR. 182. 4 PR. 253.

‖ 1 PR. 114. 3 PR. 30. 1 PR. 138.

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* 3 PR. 156.

Thirdly, If the electric ſpark be taken through common air, this air will be diminiſhed one-fourth, and a ſolution of lime, if contiguous, will be precipitated, and a ſolution of turnſole tinged red. 1 PR. 184. 186. Whence could the fixed air here produced proceed, but from the common air, and the phlogiſton of the metallic conductors? This excellent philoſopher has even ſhewn it could proceed from nothing elſe; for after that air had contributed all it could to that production, that is, was diminiſhed to the utmoſt, he changed the liquors, but could produce no change in their colour, nor the leaſt ſign of fixed air. This experiment has alſo been repeated in France, and the inſide of the glaſs tube, in which the common air was contained, was moiſtened with a ſolution of cauſtic fixed alkali, and the alkali, after the operation, was found cryſtallized; but when the tube was exhauſted of air, and the experiment repeated, no change whatſoever was found in the alkali. *Eſſai ſur l'Electricité, par Mr. Le Comte* DE LA CEPEDE, vol. I. p. 155.

Fourthly, If lead and mercury be agitated in a phial, partly filled with common air, this air will be diminiſhed one-fourth, and the reſiduum will be found completely phlogiſticated. The diminution will be ſtill greater if the phial contain dephlogiſticated air. 1 PR. 149. The lead is converted into a calx, calcination being the known effect of the amalgamation of the baſe metals; and this calx abſorbed the fixed air produced, for Dr. PRIESTLEY expelled this air from it. 1 PR. 144.; and hence an amalgama of lead and mercury decrepitates when heated*. Whence could this fixed air proceed, but from the reſpirable air? For ſurely neither lead nor mercury contain any.

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* 1 MALOUIN. 105.

If the above experiments be attended to the anſwer to the ſecond queſtion will be equally obvious. It is certain, that common air does not conſiſt of one-fourth of its bulk of fixed air; for if it did, the remaining three-fourths muſt be dephlogiſticated air: and if ſo, then the abſolute weight of a mixture of three-fourths dephlogiſticated air and one-fourth fixed air ſhould coincide at leaſt nearly with the abſolute weight of an equal bulk of common air; but in fact it is very far from it: for four cubic inches of common air weighed 1,54 gr.; but a mixture of three cubic inches of dephlogiſticated air and one of fixed air weighs 1,83 gr.; neither indeed has ſo large a portion of fixed air been ever ſuppoſed to exiſt in common air. Beſides, if fixed air pre-exiſted in common air, it might be ſeparated from it by lime-water, at leaſt in ſome degree. I have mixed one part of fixed air with twenty of dephlogiſticated air, and alſo with twenty of phlogiſticated air in cloſe veſſels, and theſe mixtures did not fail to render lime-water turbid. But let common air be agitated in lime-water ever ſo long in cloſe veſſels, not the leaſt cloudineſs will appear; nor does quick-lime, in theſe circumſtances, in the leaſt affect common air, as Dr. PRIESTLEY has obſerved. 2 PR. 184. The ſpontaneous precipitation of lime-water ariſes therefore from an accidental diffuſion of fixed air through common air, and the ſlowneſs of this precipitation ſhews its quantity to be very ſmall. The inference from the above experiments will be much ſtronger againſt the pre-exiſtence of fixed air in reſpirable air, if, inſtead of common air, dephlogiſticated air be uſed; for there the diminution is ſo great, and the quantity of fixed air produced ſo conſiderable, that it can by no means be ſuppoſed to have pre-exiſted, its properties being ſo very oppoſite to thoſe of dephlogiſticated air.

To this it has been anſwered, firſt, that fixed air in common air is united to ſome unknown baſis, which attracts it more ſtrongly than quick-lime does; but that it is precipitated from that baſis by the phlogiſton ſet looſe in phlogiſtic proceſſes, which is ſtill more ſtrongly attracted by that baſis; and, ſecondly, that the diminution both of the weight and bulk of reſpirable air in phlogiſtic proceſſes does not ariſe intirely from the ſeparation of fixed air, but from ſome other cauſe.

But neither of theſe anſwers is ſatisfactory: for the ſuppoſition of ſuch a baſis is evidently gratuitous, being ſupported by not one experiment. It is alſo contrary to analogy, there being no inſtance of the ſeparation of fixed air, nor of any other acid, from any ſubſtance, merely by the greater affinity of phlogiſton to ſuch ſubſtance. It is alſo inſufficient for the purpoſe for which it was framed; for of dephlogiſticated air 97 parts in 100 are reducible to fixed air by phlogiſtic proceſſes; and can it be imagined, that 97 parts in 100 of it were mere fixed air united to leſs than three parts of an unknown baſis? I ſay, leſs than three parts; for, according to the preſent ſuppoſition, this unknown baſis took up the phlogiſton of the ſubſtance that ſeparated the fixed air from it, and yet it, and the whole quantity of phlogiſton it took up, amounted but to three parts of an hundred; can it be ſuppoſed, that this vaſt proportion of fixed air would not in the leaſt affect lime-water, as pure dephlogiſticated air is known not to do? Can it be ſuppoſed, that ſuch an immenſe quantity of fixed air, combined with any baſis, would be ſo ſuperlatively fitted for all phlogiſtic proceſſes, while fixed air, in its diſengaged ſtate, is totally unfit for them? Beſides, this unknown baſis, after all, is nothing but phlogiſticated air, with which fixed air is incapable of contracting any union; and if its phlogiſton be waſhed away, it is not found different from common air ſlightly injured. Accordingly, we find that this conjecture, firſt advanced by Dr. PRIESTLEY in the infancy of his reſearches, is now abandoned by him. Vol. V. p. 31. And he now juſtly thinks, that common air does not contain above $\frac{1}{52}$ of its bulk of fixed air.

As to the diminution of bulk, it is certain, that the whole of it does not proceed from the ſeparation of fixed air; for though no part of the fixed air ſhould be abſorbed, yet ſince part of the common air is converted into fixed air, there muſt be a diminution of bulk, ſince fixed air is ſpecifically heavier than common air, and the bulks are inverſely as the ſpecific gravities; but the diminution of *maſs* muſt wholly, and that of bulk muſt alſo for the greater part ariſe from the abſorption of fixed air by water, or the ſubſtance from which the phlogiſton proceeds. I have ſucceſſively added ſix meaſures of nitrous air to two of dephlogiſticated air from precipitate *per ſe*, and after each addition transferred the mixture into freſh lime-water, and after each I found the lime precipitated until the whole was reduced to one-tenth nearly, ſo that nine-tenths of this dephlogiſticated air was evidently converted into fixed air; and ſince fixed air did not pre-exiſt in the dephlogiſticated air, it was evidently produced by the union of the phlogiſton of the nitrous air with the truly dephlogiſticated part of the dephlogiſticated air.

Here we ſee how fixed air is generated in moſt other phlogiſtic proceſſes, performed in common air. The phlogiſton is attracted by the dephlogiſticated part of common air, unites to it, expels part of its fire, and ſo forms fixed air; yet a part of this pure air generally eſcapes the action of phlogiſton, being protected from it by the quantity of phlogiſticated air which is always found in common air, and which forms about two-thirds of it, in the ſame manner as gold is protected by ſilver, and ſilver by gold, from the action of their reſpective menſtruums; and this is the reaſon why, in ſome phlogiſtic proceſſes, the diminution is greater than in others; and why the diminution continues to increaſe ſlowly for a long time.

Nor is the ſuppoſition, that common air conſiſts of two fluids, one phlogiſticated and the other dephlogiſticated, gratuitous; it is pointed out by ſeveral experiments. If a mixture be made of three parts phlogiſticated air and one of dephlogiſticated air, it will exactly perform the functions of common air; a candle will burn in it, an animal will live in it, juſt as in common air *. Beſides, common air may in ſome meaſure be ſeparated into theſe conſtituent parts by lying over pure water; for dephlogiſticated air is much more miſcible with water than common air, as Mr. FONTANA remarked, Phil Tranſ. 1779, p. 443. and 444 †, and SCHEELE on Fire, § 94. Hence, if common air be ſuffered to ſtand ſome time over pure water, it will be diminiſhed, the purer part being in great meaſure abſorbed by the water, and the remainder will be found to conſiſt of ſo large a proportion of phlogiſticated air that a candle will not burn in it. 1 PR. 158. 4 PR. 353. Mr. SCHEELE again expelled that part which the water had abſorbed, and found it dephlogiſticated. He alſo found, that phlogiſticated air is not at all abſorbed by water. ibid.

Hence we ſee, why the whole of any quantity of common air can never be converted into fixed air; for no part of it will unite with phlogiſton, but the dephlogiſticated part (which never exceeds one-third of the whole). This Mr. SCHEELE has deciſively proved by expoſing liver of ſulphur to a mixture

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* Mem. Par. 1777, p. 191.

† He informed me, that water takes up one-fourteenth of its bulk of dephlogiſticated air, and only one-twenty-eighth of common air.

of phlogiſticated and dephlogiſticated air; the mixture was diminiſhed in the ſame proportion as it contained dephlogiſticated air, and no more. SCHEELE, § 43.

Phlogiſticated air, therefore, is not the uſual product of common phlogiſtic proceſſes; but the phlogiſticated reſiduum that is found after ſuch proceſſes muſt have pre-exiſted, as that evidently does which is found after the mixture of nitrous and very pure dephlogiſticated air, for almoſt the whole of this laſt is turned into air which is abſorbed by water, and precipitates lime, as we have already ſeen, ſo that no part of it is converted into phlogiſticated air, this being immiſcible with water. Now common air is affected by nitrous air juſt in the ſame manner, and differs only in degree; therefore the phlogiſticated air, which is found after its phlogiſtication in the uſual proceſſes, was not produced by thoſe operations, but pre-exiſted.

Phlogiſticated air conſiſts of fixed air ſuper-ſaturated with phlogiſton, as ſulphur does of volatile vitriolic acid ſuperſaturated with phlogiſton; and as ſulphur is not generally formed when the vitriolic acid unites to phlogiſton, but only volatile vitriolic acid, ſo neither is phlogiſticated air each time that pure air unites to phlogiſton, but rather fixed air. I ſay *ſuper-ſaturated*, becauſe it contains ſuch a quantity of phlogiſton as to be inſoluble in water. Many experiments of Dr. PRIESTLEY clearly point out this compoſition. Thus that celebrated philoſopher has found, that if phlogiſticated air be agitated in water, out of which its air had been boiled, and whoſe ſurface is expoſed to the atmoſphere, it will be in great meaſure purified (juſt as ſulphur is decompoſed by trituration in water), and if then it be paſſed through lime-water two or three times, it renders it turbid. 2 PR. 218. Here then, the exceſs of phlogiſton, by reaſon of its repulſion from water, is eaſily attracted by the dephlogiſticated part of the common atmoſphere, which is immediately imbibed by the water out of which its air had been boiled; the phlogiſticated air is thereby decompoſed and partly turned into fixed air, which makes the lime-water turbid. Part alſo of the fixed air is decompoſed as will preſently be ſeen, and hence the degree of purity which it acquires. Further, if the electric ſpark be taken in fixed air, three-fourths of it will be rendered inſoluble in water, and the whole will probably become ſo if the operation be long enough continued. 1 PR. 248. This inſoluble reſiduum Mr. FONTANA found to be phlogiſticated air; and that, if this phlogiſticated air be agitated in water (whoſe ſurface is expoſed to the atmoſphere) it will become again common air. *Recherches Phyſiques*, 77. That is, it will acquire a degree of purity nearly the ſame as that of common air. This fully confirms all that has been hitherto ſaid concerning theſe airs; ſo alſo, if a mixture of filings of iron and ſulphur, made into a paſte, be expoſed to fixed air, and made to ferment, part of the fixed air will be turned into phlogiſticated air, 3 PR. 257. Juſt as in another equally curious experiment he found, that vitriolic air was converted into ſulphur by the gradual exhalation of phlogiſton from a ſolution of that air in water, and as it daily happens in the hot baths at Aix la Chapelle. And hence we ſee, that fixed air, even in its elaſtic ſtate, is capable of taking an exceſs of phlogiſton, when this laſt is inſenſibly ſeparated from any ſubſtance, and then becomes phlogiſticated air. Phlogiſticated air may alſo be formed by a rapid and copious affluence of phlogiſton, in certain circumſtances, as we ſhall ſoon ſee. I ſhould not omit, that phlogiſticated air, after it has been purified from phlogiſton by agitation in water, is again diminiſhable by phlogiſtic proceſſes, and that fixed air is precipitated

pitated from it as uſual. 2 PR. 219. A circumſtance which at that time was thought inexplicable, and which indeed is ſo, on any other principles but thoſe here laid down, of which it is an immediate conſequence.

Having thus far ſynthetically proved the conſtituent parts of fixed air to be pure elementary air and phlogiſton, I ſhall now endeavour to do the ſame by its analyſis: and, in the firſt place, that it contains phlogiſton, and even in ſuch quantity as to deſerve to be claſſed among the phlogiſticated acids, appears by its action on *black mangancſe*. This ſemi-metallic calx, as has been proved by that admirable chemiſt Mr. SCHEELE, is completely ſoluble only in phlogiſticated acids, and is precipitable from them by fixed alkalies in the form of a white calx. He alſo found, that this mangancſe is alſo ſoluble in water ſtrongly impregnated with fixed air, and is alſo precipitable from it in the form of a *white calx*. 35 Mem. Stock. p. 96.

If fixed air be repeatedly diſſolved in, and expelled from water, it leaves each time a reſiduum which is inſoluble in water, diminiſhable by nitrous air, and capable of ſupporting animal life. Hence it is evidently decompoſed, the phlogiſton ſeparating from it, and gradually uniting to the common atmoſphere by reaſon of the repulſive power betwixt it and water. Dr. PRIESTLEY indeed found, that a candle would not burn in it; but this ariſes only from a mixture of a ſmall quantity of fixed air not yet decompoſed, of which, according to the experiments of Mr. CAVENDISH, one-ninth is ſufficient to extinguiſh a candle.

Again, Mr. ACHARD has converted fixed air into air of nearly the ſame purity as common air by paſſing it five or ſix times through melted nitre. Mem. Berlin. 1778. Mr. CAVALLO paſſed it but once through melted nitre, and yet found it conſiderably meliorated, for it was diminiſhed by nitrous air. In this caſe the nitrous acid attracted the phlogiſton; for it is known to become phlogiſticated by the fuſion of nitre, ſo as to be expellable even by the vegetable acids. 2 N. Act. Upſ. 171. And aqua regia may be made by mixing nitre with marine acid.

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* 1 PR. 34, 40. 2 PR. 219, 220.

I ſhall now briefly conſider what may be ſaid in oppoſition to this doctrine.

In the firſt place it may be difficult to conceive, that the addition of phlogiſton ſhould render any ſubſtance more ſoluble in water, as it is known to render moſt acids leſs ſoluble in that liquid; but a little attention will ſhew, that phlogiſton does not always render ſubſtances leſs ſoluble in water than they were before; for the acid of ſugar is leſs ſoluble in water than ſugar itſelf, though ſugar conſiſts of that acid united to phlogiſton. The dephlogiſticated marine acid unites more difficultly with water than the ſame acid does when phlogiſticated, as the illuſtrious BERGMAN has obſerved*. Cauſtic volatile alkali has been decompoſed by Mr. SCHEELE, and found to conſiſt of an air inſoluble in water, and phlogiſton; ſo that it is rendered ſoluble in water only by union with phlogiſton. It would be foreign to the ſubject to enter into the reaſon of theſe exceptions, but the facts are certain.

Another objection may be drawn from a remarkable experiment to be found in the fifth volume of Dr. PRIESTLEY's obſervations, where it is ſaid, that inflammable air and common air being fired by an electric ſpark over lime-water, the diminution took place all once, and the lime was not precipitated; but as it is equally true, that fixed air is precipitated by other phlogiſtic proceſſes, this experiment proves only, that in theſe particular circumſtances, where a large quantity of phlogiſton is ſuddenly heated and transferred all at once upon the dephlogiſticated part of common air, phlogiſticated air may be formed as ſulphur and volatile acid is formed, when a large quantity of hot phlogiſton is united all at once to the vitriolic acid.

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* Anleitung, § 335.

Analogous to this is another experiment of Mr. CAVALLO's, where he found, that, by the exploſion of gunpowder, a large quantity of phlogiſticated air is produced*; and alſo another of Dr. PRIESTLEY's, wherein he found, that by firing a mixture of equal parts, ſulphur and nitre, only one-twelfth of the air produced was fixed air, the remainder being phlogiſticated air. But I own the circumſtances of the former experiment are not as yet well known to me; not having been able to repeat it in ſuch a manner as to remove all doubt either of the eſcape of the air through the cement which fixes the wire that conducts the electric fire, during combuſtion; or that the ſmall quantity of inflammable air uſed prevents the fixed from being ſenſible. It may alſo happen, that to the production of fixed air it is neceſſary that the phlogiſton be condenſed to a certain degree, as it is in common caſes; and perhaps, when exceedingly rarified, as it is in inflammable air from metals, it forms ſome other, as yet unknown, compound. Thus much is certain, that all other inflammable air, fired with the electric ſpark, produces fixed air; and all other inflammable air is ſpecifically heavier than the metallic, and before inflammation evidently contains no fixed air. Mr. WARLTIRE, after burning metallic inflammable air, found a *white powdery ſubſtance* (probably a calx) which may have abſorbed the fixed air.

However, in the common proceſs of combuſtion of animal and vegetable ſubſtances it is certain, that fixed air is ſeparated from common air, and that the whole diminution is owing to its production and abſorbtion. Mr. LAVOISIER has ſet theſe points in the cleareſt light. He introduced a lighted candle into a receiver ſtanding on mercury: the air at firſt expanded by reaſon of the heat, and the candle was ſhortly after extinguiſhed; but when all was cold, there was ſcarcely any diminution. He then introduced, under the receiver, a cauſtic fixed alkaline liquor: the air was immediately diminiſhed, and the diminution reached nearly one-ninth of the whole. He then introduced a ſmall quantity of vitriolic acid; the alkali immediately efferveſced, gave out its fixed air, the mercury redeſcended, and the air in the receiver occupied the ſame ſpace as at firſt; ſo that this experiment is perfectly concluſive. He alſo lighted a candle in dephlogiſticated air, and when it was extinguiſhed, introduced a cauſtic fixed alkaline liquor, and then, and only then, this air was diminiſhed two-thirds, by which it is evident, that two-thirds of it were converted into fixed air; but the remaining third was ſo far from being phlogiſticated air, that a candle burned in it as well as ever, and after it went out half of this air was abſorbed by a cauſtic fixed alkali, and the remainder was ſtill little worſe than common air. Mem. Par. 1777, p. 195, &c.

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* CAVALLO on Air, p. 812.

Yet Mr. LAVOISIER thinks, that by the calcination of metals fixed air is not produced; but that the metals abſorb the dephlogiſticated part of common air, and are thereby converted into a calx. And on this is founded his extraordinary opinion of the non-exiſtence of phlogiſton; whereas it is evident, that even mercury affords inflammable air, and conſequently contains phlogiſton, and that it loſes part of this during calcination; and conſequently fixed air muſt be produced, as he himſelf acknowledges it to be during combuſtion, by the union of inflammable air and the dephlogiſticated part of common air, which after this union is abſorbed by the calx. It is true, that the mercurial calx, and alſo the calces of lead, and many others, yield dephlogiſticated air; but then the mercury is always revived, ſo that it is evident, it retakes the phlogiſton from the fixed air, of which nothing then remains but the dephlogiſticated part, which accordingly appears in the form of dephlogiſticated air. Dr. PRIESTLEY never found the whole of the mercury revived, and accordingly he recovers a little fixed air from the mercurial calx. 2 PR. 217. But Mr. LAVOISIER finds the whole of the mercury revived, and for that reaſon finds no fixed but all dephlogiſticated air; thus their different reſults are clearly explained, and probably proceed from the different degrees of heat they employed, and the different phlogiſtication of their acids. The dephlogiſticated air that is extracted from *minium* proceeds alſo from a partial reviviſication of the lead, which always takes place*: nor is it wonderful, that this calx ſhould dephlogiſticate fixed air, ſince it dephlogiſticates the marine acid alſo, as Mr. SCHEELE has obſerved†.

To this it will probably be objected, that dephlogiſticated air muſt pre-exiſt in the *minium*, ſince it is expelled by the marine acid; but this does not follow; for if manganeſe be diſſolved in the common marine acid which is phlogiſticated, and afterwards expelled from it by the vitriolic, it will alſo be found dephlogiſticated.

I ſhall now proceed to inveſtigate the proportion of phlogiſton and elementary or reſpirable air in fixed air.

Dr. PRIESTLEY, in the fourth volume of his Obſervations, p. 380. has ſatisfactorily proved, that nitrous air *parts* with as much phlogiſton to common air as an equal bulk of inflammable air does when fired in the ſame proportion of common air. Now, when inflammable air unites with common air, its whole weight unites to it, as it contains nothing elſe but pure phlogiſton; ſince, therefore, nitrous air phlogiſticates common air to the ſame degree that inflammable air does, it parts with a quantity of phlogiſton equal to the weight of a volume of inflammable air ſimilar to that of nitrous air. Now 100 cubic inches of inflammable air weigh 3,5 gr.; therefore, 100 cubic inches of nitrous air part with 3,5 gr. of phlogiſton when they communicate their phlogiſton to as much common air as will take it up. I ſay, that nitrous air *parts* with *as much* phlogiſton, becauſe it is certain, that it does not part with the *whole* of its phlogiſton to common or dephlogiſticated air, for it contains much more, as already ſhewn, and, as appears by the red colour, it conſtantly aſſumes when mixed with common or dephlogiſticated air, which colour belongs to the nitrous acid combined with its remaining phlogiſton, and not to the fixed air then produced, nor to the phlogiſticated air remaining, as is very evident. Hence the acid, thus formed, is volatile. 4 PR. 267.

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* BEAUME, 7. 1 Pott. Lithog. 29. 3 Dict. Chy. 205.

† Kon. Vet. Acad. Handling. vol. XXXV. p. 193.

One meaſure of the pureſt dephlogiſticated air and two of nitrous air occupy but $\frac{3}{100}$th parts of one meaſure, as Dr. PRIESTLEY has obſerved, vol. IV. p. 245. Suppoſe one meaſure to contain 100 cubic inches, then the whole very nearly of the nitrous air will diſappear, its acid uniting to the water over which the experiment is made, and 97 cubic inches of the dephlogiſticated air, which is converted into fixed air by its union with the phlogiſton of the nitrous air; therefore 97 cubic inches of dephlogiſticated air take up all the phlogiſton which 200 cubic inches of nitrous air will part with; and this we have found to be ſeven grains; therefore, a weight of fixed air equal to that of 97 cubic inches of dephlogiſticated air and 7 of phlogiſton, will contain 7 gr. of phlogiſton. Now, 97 cubic inches of dephlogiſticated air weigh 40,74 gr.; to which, adding 7 gr. we have the whole weight of the fixed air equal 47,74 gr. = 83,755 cubic inches; and conſequently 100 *cubic inches of fixed air contain 8,357 gr. of phlogiſton, and the remainder elementary air.*

100 *gr. of fixed air contain* 14,661 *of phlogiſton and* 85,339 *of elementary air*; which, when ſtripped of phlogiſton, and impregnated with its proper proportion of elementary fire, becomes again dephlogiſticated air. Hence alſo, 100 cubic inches of dephlogiſticated air are converted into fixed air by 7,2165 gr. of phlogiſton, and will be then reduced to the bulk of 86,34 cubic inches.

And reciprocally, 100 cubic inches of fixed air, being decompoſed, will afford 115,821 cubic inches of dephlogiſticated air, and part with 7,2165 gr. of phlogiſton, ſuppoſing the decompoſition to be complete; that is, the dephlogiſticated air abſoutely pure.

Having read the foregoing account of the nature of fixed air to Dr. PRIESTLEY, I had the ſatisfaction to find it met with his entire approbation, which he authorized me to mention, notwithſtanding what he had advanced to the contrary in his laſt publication.

OF

OF THE QUANTITY OF PHLOGISTON IN VITRIOLIC AIR.

The method I purſued was this:

1ſt, I found the quantity of nitrous air a given weight of copper afforded when diſſolved in the dephlogiſticated nitrous acid, and by that means how much phlogiſton it parts with.

2dly, I found the quantity of copper which a given quantity of the dephlogiſticated vitriolic acid could diſſolve; and obſerved, that it could not diſſolve the greateſt quantity of copper without dephlogiſticating a further quantity which it does not diſſolve.

3dly, I found how much it dephlogiſticates what it thoroughly diſſolves, and how much it dephlogiſticates what it barely calcines.

4thly, How much inflammable air a given quantity of copper affords when diſſolved in the vitriolic acid to the greateſt advantage.

5thly, I deduct from the whole quantity of phlogiſton expelled by the vitriolic acid the quantity of it contained in the inflammable air; the remainder ſhews the quantity of it contained in the vitriolic air.

The particulars were as follows:

1ſt, 100 gr. of copper diſſolved in the dephlogiſticated nitrous acid afforded me 67,5 cubic inches of nitrous air, which, according to the before mentioned calculation, contain 4,52 gr. of phlogiſton.

2dly, 100 gr. of real vitriolic acid take up or diſſolve 54,73 of copper, and 100 gr. of copper require about 182,714 gr.

of real vitriolic acid to diſſolve them. Again, 100 gr. of copper, when diſſolved in the vitriolic acid, retain only as much phlogiſton as is contained in three cubic inches of nitrous air, that is, 0,2 of a grain; therefore, ſince 100 gr. of copper give out 4,52 of phlogiſton, the vitriolic acid ſtrips it of 4,52 − 0,2, that is, 4,32 gr. of phlogiſton.

3dly, To diſſolve 70 gr. of copper in the vitriolic acid, to the greateſt advantage, 20 more muſt be ſlightly dephlogiſticated; therefore, to diſſolve 100 gr. of copper in this acid, 28,6 more muſt be ſlightly dephlogiſticated. 8 grs. of this ſlightly dephlogiſticated calx afforded 4 cubic inches of nitrous air; therefore, 28,6 would afford 14,3, which contain 0,958 gr. of phlogiſton; but 28,6 gr. of copper, before any dephlogiſtication, contain 1,292 gr. of phlogiſton; therefore, they loſe by this ſlight dephlogiſtication 0,344 of a grain of phlogiſton. Hence, when 100 gr. of copper are diſſolved in the vitriolic acid, the quantity of phlogiſton expelled is 4,32 + 0,34 = 4,66 gr.

4thly, The quantity of inflammable air afforded by the moſt advantageous ſolution of 100 gr. of copper in the vitriolic acid is 11 cubic inches, which amount to 0,385 of a grain of phlogiſton

5thly, The ſolution of 100 gr. of copper in the vitriolic acid afforded over mercury 75,71 cubic inches of air; but of this only 11 cubic inches were inflammable air, the remainder therefore was vitriolic acid air, amounting to 64,71 cubic inches.

6thly, Then the whole quantity of phlogiſton expelled during the ſolution of 100 gr. of copper in the vitriolic acid is 4,66 gr.; of this inflammable air contains but 0,385 of a grain: the remainder therefore, which conſiſts of 4,275 gr. muſt be contained in the 64,71 cubic inches of vitriolic air: therefore, 100 *cubic inches of vitriolic air contain* 6,6 *gr. of phlogiſton, and* 71,2 *gr. of acid,* and 100 cubic inches of this air weighing 77,8 gr. 100 *gr. of this air contain* 8,48 *gr. of phlogiſton and* 91,52 *of acid.*

OF THE QUANTITY OF PHLOGISTON IN SULPHUR.

This I endeavoured to find by eſtimating the quantity of fixed air produced during its combuſtion.

To the top of a glaſs bell, which was open, I firmly tied and cemented a large bladder, deſtined to receive the air expanded by combuſtion, a quantity of which generally eſcapes when this precaution is not uſed. Under this bell, which contained about 3000 cubic inches of air, I placed a candle of ſulphur, weighing 347 gr.; its wick (which was not conſumed) weighed half a grain: it was ſupported by a very thin concave plate of tin, to prevent the ſulphur from flowing over during the combuſtion, and both were ſupported by an iron wire, fixed on a ſhelf in a tub of water. As ſoon as the ſulphur was fired with a very feeble flame, it was covered with the bell, the air being ſqueezed out of the bladder. The inſide of the bell was ſoon filled with white fumes, ſo that the flame could not be ſeen. In an hour after, the fumes thoroughly ſubſided, and all was cold. The water roſe within the bell to a height equal to 87,2 cubic inches; whence I deduce that 87,2 cubic inches of fixed air were produced, which contain 7,287 gr. of phlogiſton, which ſeparated from the vitriolic acid, and united to the dephlogiſticated part of the common air under the bell.

The candle of ſulphur being weighed, was found to have loſt 20,75 gr.; therefore, 20,75 gr. of ſulphur contain 7,287 gr. of phlogiſton, beſides the quantity of phlogiſton which remained in the vitriolic air. This air muſt have amounted to $20,75 - 7,287 = 13,463$ gr. which contain 1,141 gr. of phlogiſton; therefore, the whole quantity of phlogiſton in 20,75 gr. of ſulphur is 8,428 gr.; therefore, 100 *gr. of ſulphur contain* 40,61 *gr. of phlogiſton and* 59,39 *of vitriolic acid.*

Several attempts have hitherto been made to determine the proportion of the conſtituent parts of ſulphur; but all were evidently defective. The firſt was that of STAHL, who calculated the quantity of phlogiſton from that of the acid remaining after ſlow combuſtion; but as much, both of acid and phlogiſton, was diſſipated, and as the remaining acid was alſo phlogiſticated, and attracted much of the moiſture of the air, no concluſion whatever could be drawn from this experiment. The ſecond method was, to form a liver of ſulphur, and convert this by a gentle long continued heat into a tartar vitriolate, and then calculate the weight a given quantity of alkali would gain by this operation. This was alſo deviſed by STAHL, and followed by BRANDT and NEWMAN, and by it they determined the proportion of phlogiſton to that of acid to be nearly as 1 to 16. But during the formation of the liver of ſulphur, whether in the moiſt or dry way, much of the phlogiſton and acid is diſſipated, as is evident by the vapour and ſmell that proceed from it, their alkali alſo contained fixed air, which it loſt during the operation, and of which they kept no account, as they were ignorant of its exiſtence; and the tartar vitriol formed by them or ſal polycreſte retained much undecompoſed ſulphur, as always happens when it is not ſtrongly heated; ſo that this method alſo was very imperfect, however ſome ſubſequent chemiſts who made the experiment with more care concluded from it, that ſulphur contained one-ſeventh of phlogiſton. EXLEBEN, § 760.

By weighing flowers of ſulphur in a perforated braſs box in water, I found its ſpecific gravity to be 1,924. It remained in the water a quarter of an hour before any air iſſued from it, and then ſome bubbles aroſe; but when I opened the box, I found the middle part of the flowers quite dry, ſo that I make no doubt but ſome air ſtill remained, and that its ſpecific gravity is ſtill greater. Mr. PETIT weighed it in oil, and found its ſpecific gravity 2,344, which I believe to be nearly the truth.

OF THE QUANTITY OF PHLOGISTON IN MARINE ACID AIR.

8 gr. of copper diſſolved in colourleſs ſpirit of ſalt afforded but 4,9 cubic inches of air, when the air was received over water, and this air was inflammable.

8,5 gr. of copper being diſſolved in the ſame quantity of the ſame ſpirit of ſalt, and the air received over mercury, afforded 91,28 cubic inches of air; but of theſe only 4,9 cubic inches were inflammable air; the remainder, therefore, *viz.* 86,38 were marine air, which weigh 56,49 gr.

Now, as ſpirit of ſalt certainly does not dephlogiſticate copper more than the vitriolic acid does, it follows, that theſe 4,9 cubic inches of inflammable air, and 86,38 cubic inches of marine air, do not contain more phlogiſton than would be ſeparated from the ſame quantity of copper by the vitriolic acid: and ſince 100 grains of copper would yield to the vitriolic acid 4,32 gr. of phlogiſton, 8,5 gr. of copper would yield 0,367 of a grain of phlogiſton; this then is the whole quantity extracted by the marine acid, and contained in 91,28 cubic inches of air, and deducting from this the quantity of phlogiſton contained in 4,9 cubic inches of inflammable air (=0,171 of a grain), the remainder, *viz.* 0,367 − 0,171 = 0,196 is all the phlogiſton that can be found in 86,38 cubic inches of marine air. Then 100 *cubic inches of marine air can contain but* 0,227 *nearly of a grain of phlogiſton* 65,173 *of acid.*

Hence we ſee why it acts ſo feebly on oils, ſpirit of wine, &c. having a very ſmall affinity to phlogiſton; and why it is not diſlodged from any baſis by uniting with phlogiſton, as the vitriolic and nitrous acids are, its affinity to it being inconſiderable.

XVI. *Del modo di render ſenſibiliſſima la più debole Elettricità ſia Naturale, ſia Artificiale. By Mr.* Alexander Volta, *Profeſſor of Experimental Philoſophy in* Como, *&c. &c.; communicated by the Right Hon.* George *Earl* Cowper, *F. R. S.*

Read March 14, 1782.

UN apparechio, che portando a uno ſtraordinario ingrandimento i ſegni elettrici fa ſi, che oſſervabile divenga e coſpicua quella virtù, che altrimenti per l'eſtrema ſua debolezza sfuggirebbe i noſtri ſenſi, ognun comprende di quale e quanto vantaggio ſia per riuſcire nelle ricerche ſull' elettricita, e maſſime intorno alla naturale atmosferica, la quale, come ſappiamo, non in ogni tempo, anzi aſſai di rado, allora ſolamente cioè che il cielo è ingombro di nuvoloni ſcuri e tempeſtoſi, avvienne che ci renda ſenſibile ne' conduttori ordinari non molto elevati, e appena è che in altri tempi ne moſtri qualche indizio in quelli elevatiſſimi, o ne' *cervi volanti* portati all' altezza di più centinaja di braccia. Or un tale apparecchio, mercè di cui un conduttore atmosferico, anche di non grande elevazione, vi dia ſegni ad ogn' ora e in ogni coſtituzione di tempo, molto chiari e diſtinti di quel qualſiſia picciolo elettrizzamento che in lui induce l'atmosfera, ecco io ve lo preſento nel mio elettroforo: in quella ſemplice macchina, che è ormai nelle mani di tutti, e che ſe altro pregio pur non aveſſe verrebbe abbaſtanza raccomandata agl' Elettriciſti per queſto che lor offre facile mezzo di ſpiare la più languida e impercettibile elettricità ſi naturale che artificiale, con tirarla ſopra di sè, ed accumularla al punto di promoverne e invigorirne per ſingolar maniera i ſegni.

2. In vero ogni volta che queſti mancano nell' ordinario modo di ſperimentare, e ne ſcintilla ſcorgeſi nè cenno benchè minimo di attraimento, il dire che pur vi ſia elettricita, fora un' aſſerzione gratuita, anzi un giudicare contro ogni apparenza. Malgrado queſto non poſſiamo neppur dire accertatamenta che punto non ve n' abbia: e il concluderlo da ciò ſolo che niun ſegno per anco ci ſi moſtra, è un precipitare il giudizio; imperocchè chi ci aſſicura che qualche elettricità ivi non ſi truovi realmente, ma coſi debole da non poter attrarre tampoco un legger filo? Or queſto è che c'importa in molti caſi di ſapere, ſpecialmente quando ſi tratta di elettricità naturale. Un conduttore atmoſferico poco elevato non dà ordinariamente ſegni, come già ſi è detto, che quando gli ſovraſta oſcuro nembo: a cielo coperto d'alte nubi ſparſe o diſteſe equabilmente, quando l'aria è ingombrata da nebbie, in tempo di pioggia placida ed anche dirotta, tranne qualche roveſcio improvviſo, raro è che ſcorger vi ſi poſſa alcun indizio di elettricità, o nulla mai a ciel ſereno, ſia placido, ſia ventoſo. Stando pertanto alle apparenze, e al giudizio di un elettroſcopio comune, anche dè più ſenſibili, direbbeſi che il conduttore non è elettrizzato punto, e che per conſeguenza non domina elettricità di ſorta ne' campi dell' aria poco alti ove quel conduttore porta la teſta. Eppure non è così: un altro elettroſcopio di gran lunga migliore qual veramente puo dirſi il noſtro apparechio, giacchè ne adempie con tanto vantaggio le funzioni, ci fa vedere che da qualche elettricità è pur ſempre inveſtito quel conduttore, avvegnache ne ſi moſtri di per ſè affatto inerte: ci fa, dico, vedere e toccar con mano ch' egli non ne è mai privo affatto; onde convien giudicare in egual modo che non ne è mai priva l'aria che lo circonda. Ed ecco come

reſtiamo convinti che anche alla più baſſa regione dell' atmoſfera, e fino a pochi piedi da terra s'eſtende l'azion coſtante e perenne dell' elettricità naturale. Cotal elettricità ſebbene inſenſibile rimanga finche da quel tratto d'atmosfera ſi comunica ſoltanto al detto conduttore; ove poi per mezzo di lui ſi comunichi inſiememente all' elettroforo noſtro, ſi raccorà entro a queſto più facilmente, e in maggiore copia*; ſi è per tal modo, che ſorger quindi potranno i noti ſegni di attrazzione e di ripulſione ſenſibili abbaſtanza per dinotarci ſenza equivoco non che l'eſiſtenza, la ſpecie ancora dell' elettricità, cioè ſe *poſitiva* o *negativa*. Che più? non mancherà talora di comparire perfino qualche ſcintilluzza. Ogniqualvolta poi il conduttore deſſe già di per ſe qualche ſegno, movendo alcun poco un legger filo, aſpettatevi pure col' ſoccorſo del' noſtro apparecchio, ſcintille pungenti, e ogn' altro ſegno vigoroſiſſimo.

3. Mà veniamo ſenza più al modo di far ſervire all' intento cotal apparecchio, a cui in queſto caſo meglio che il nome che altronde porta di elettroforo, l'altro già indicato di *elettroſcopio*, anzi pure quello di micro-elettroſcopio potrebbe convenire. Ma io amo meglio di chiamarlo *condenſatore* dell' elettricità, per uſare un termine ſemplice e piano, e che eſprime a un tempo la ragione e il modo dè fenomeni di cui ſi tratta, come vedraſſi nella 2ª parte del preſente ſcritto. Tutto dunque ſi riduce a queſte poche operazioni.

(A) Convien prendere un piatto d'elettroforo, che abbia l'incroſtatura di reſina aſſai ſottile, e a cui o non ſia ſtata dianzi impreſſa alcuna elettricità, o ſe mai vi e ſtata, vi ſi ſia ſpenta affatto.

(B) A queſta faccia reſinoſa immune da ogni elettricità ſi ſoprapponga convenientemente il ſuo ſcudo (coſi io chiamo la lamina ſuperiore dell' elettroforo): voglio dire le ſi applichi a combaciamento, e ſi collochi nel bel mezzo in modo, che non tocchi in alcun punto l'orlo metallico del piatto, ma rimanga iſolato.

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* Come ciò ſegua ſi ſpiegherà nella 2ª parte di queſta memoria.

(C) Coſi congiunti eſſendo ſi adattino ſotto al filo conduttore dell' elettricità atmosferica in guiſa, che lo ſcudo venga toccato dove che ſia dal detto filo, egli ſolo lo ſcudo, e in niun modo il piatto.

(D) In queſta ſituazione ſi laſcino le coſe per un certo tempo, finche lo ſcudo poſſa aver raccolta competente doſe di quell' elettricità, che dal filo conduttore gli s'iſtilla lentiſſimamente.

(E) Da ultimo ſottraggaſi al contatto e influſſo del filo conduttore lo ſcudo tuttavia unito al ſuo piatto; indi ſi diſgiunga anche da queſto, levandolo in alto al conſueto modo per il ſuo manico iſolante: e allora ſarà che ſe ne otterranno gl' aſpettati ſegni coſpicui di attrazione, di ripulſione, e di qualche ſcintilla eziandio, di pennoncelli &c. nel tempo che il conduttore di per ſè non giugna a moſtrar nulla, o appena un' ombra di elettricità.

4. Ho detto (prec. D) che il filo conduttore debbe toccare lo ſcudo per un certo tempo. Quanto però non è facile il determinarlo, dipendendo dalle circoſtanze. Talora vi abbiſogneranno 8. 10. e più minuti; quando cioè il conduttore da par ſè ſolo non fà vedere il minimo ſegno di elettricità: altre volte più poco. Che ſe un debole indizio pur vi compariſſe, tantoche un legger filo faceſſe cenno d'eſſerner attratto, baſteria in tal caſo laſciar in contatto di eſſo conduttore il noſtro ſcudo ſol pochi ſecondi, per abilitar queſto a dar ſegni molto vivaci.

5. Una coſa ſi vuol oſſervare riſpetto al filo conduttore medeſimo, ed è ch' egli ſia ben continuo, o ſe è poſſibile d'un pezzo ſolo dall' alto fino al baſſo, dove viene a comunicare collo ſcudo: cioè ſi dee evitare aſſolutamente ogni interruzione, e il

più che ſi puo ancora la ſemplici giunture ad anello od uncino, per la ragione che ciaſcuna di tali giunture portando un qualche impedimento al paſſagio dell' elettricità, avvenir puo che quella, che contrae il conduttore in alto, s'arreſti, nè giunga al luogo deſiderato, cioè fino allo ſcudo. Così ſuccederà diffatti ogni qualvolta l'elettricità è deboliſſima, ſe in luogo d'un filo metallico continuo, una catena di più annelli da quello pendente venga a toccare coteſto ſcudo. Non ſi creda per queſto che una ſola giuntura o due poſſano egualmente impedire la riuſcita; ma ne verra ſempre del pregiudizio: e qualora l'elettricità foſſe eſtremamente debole, potrebbe sì per l'indicato difetto mancare del tutto l'eſperimento.

6. Riguardo all' elettroforo da adoperarſi altre oſſervazioni rimangono, di cui ora mi convien parlare. E la prima accennata ſopra al § 3. (let. A) ſi è che lo ſtrato reſinoſo importa molto che ſia ſottile, avendo io ſempre provato che quanto più lo è tanto maggior doſe di elettricità permette, anzi fa che ſi raccolga entro allo ſcudo cui porta indoſſo, di quell' elettricità, dico che gli s'infonde o dall' atmosfera per mezzo del filo conduttore, o da qualſivoglia altra potenza elettrica. Se foſſe pertanto ſteſa la reſina alla ſpeſſezza d'un quarto di linea, o non maggiore di una mano di varnice, riuſcirebber le prove ottimamente; ſiccome all' incontro eſſendo groſſa un pollice o più, andrebber le coſe maliſſimo.

7. In ſecondo luogo la ſuperficie di eſſa reſina debb' eſſere quanto ſi puo piana e liſcia, e piana e liſcia ſimilmente l'inferior faccia dello ſcudo, ſicchè vengano a combaciarſi bene (ivi let. B). E noto quanto un miglior combaciamento favoriſca gli effetti dell' elettroforo; ond' ebbi ben ragione di raccommandar queſta come una delle principali condizioni nella deſcrizione che pub-

blicai a ſuo tempo di queſta machina[*]. Ma è ancor più grande l'influenza, che l'ampio e perfetto contatto ha ſopra l'iſteſſo apparecchio, allorchè il medeſimo agiſce in qualità di condenſatore.

8. Da ultimo merita particolar attenzione quanto alla già citata let. A ſi è preſcritto, che alla faccia reſinoſa, cui ſi applica lo ſcudo, non dee eſſere impreſſa alcuna elettricità. La ragione per cui vuolſi che ne ſia affatto priva ella è, che altrimenti i ſegni dello ſcudo, allorchè s'alza, diverrebbero equivoci, non eſſendo più la ſola elettricità trasfuſa in eſſo ſcudo dal conduttore atmosferico quella che giuoca, ma inſieme anche l'altra occaſionata dall' elettricità impreſſa ed inerente alla faccia reſinoſa: quando a noi importa di eſplorare la ſola ſopraveniente la detto ſcudo.

Se dunque la faccia reſinoſa del piatto, di cui volete ſervirvi, è rimaſta ſempre intatta, va bene. Ma ſe è ſtata già eccitata, e vi ſi mantiene tuttavia parte dell' impreſſa elettricità, egli convien fare di tutto per iſpegnarla; cio che non è ſi agevol coſa. Il paſſarvi ſopra un panno alquanto umido, applicandolo ben bene a tutta la ſuperficie, è un de mezzi più efficaci ch' io mai abbia trovato[†]; pur non toglie talvolta che dopo qualche tempo lo ſcudo poſatovi ſopra, e previo il ſolito toccamento, rialzato, non attragga ſenſibilmente un filo. Lo ſteſſo ſuccede non di raro anche dopo aver tuffato tutto il piatto nell' acqua, laſciatovelo un pezzo, e quindi fattolo raſciugare all' aria, Lo ſquagliare la ſuperficie della reſina al fuoco o al ſole, è forſe il più ſicuro ſpediente per farne ſvanire tutta quanta l'elettricità, ſicchè non ne rimanga pur ombra o veſtigio nella ſteſſa reſina, raſſodata che ſia *. Una maniera più ſpedita è di far paſſare ſopra tutta la faccia della reſina la fiamma di una candela, o d'un foglio di carta acceſo. A qualunque però di tai mezzi uno ſi appigli, per accertarſi che l'elettricità ſia ſpenta a ſegno che più non poſſa aver parte alcuna l'azione propria dell' elettroforo agli effetti che riſultar debbono unicamente dall' elettricità infuſa allo ſcudo dal conduttore atmosferico, converrà far prima la prova ſe poſato eſſo ſcudo ſulla faccia reſinoſa, toccato col dito, e rialzato al conſueto modo, non mova neppure un ſottiliſſimo pelo: allora non producendo alcun effetto in qualitá d'elettroforo, ſervirá ottimamente all' altr' uſo, cui vien deſtinato, di condenſatore dell' elettricità.

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[*] Si truova queſta deſcrizione in un colle principali eſperienze, e un piccol ſaggio di ſpiegazione, in due memorie indirizzate in forma di lettera al Dr. PRIESTLEY, e pubblicate in un' opera periodica di Milano intitolata *Scelta d'Opuſcoli intereſſanti*, per l'anno 1775.

[†] Vegg. l'accennata deſcrizione dell' elettroforo.

9. Se mi ſi dimandaſſe ora a qual grado giunga nel deſcritto apparecchio cotal condenſazione dell' elettricità, cioè a quanto maggior forza ſorger poſſano i ſegni elettrici nello ſcudo quando s' alza, riſponderei che non è facile il determinarlo, dipendendo da molte circoſtanze. Che però, le altre coſe pari, l'aumento è maggiore in ragione che il corpo che forniſce l'elettricità allo ſcudo ſi truova avere maggiore capacitá; ed è più grande in proporzione che la forza elettrica im-

* E ſtato creduto per molto tempo che il calore, e molto più la liquefazione del ſolfo e delle reſine, baſtaſſe ſenz' altro ad eccitarvi l'elettricità. Ma tranne la tormalina, ed alcune altre pietre, che ſí veramente concepiſcono l'elettricità pel ſolo calore, le reſine e il ſolfo non è mai che lo facciano, ſe loro non ſopravvenga qualche ſtropicciamento, o tocco almeno d'altro corpo. L'errore e nato, come ha avvertito il Prof. BECCARIA con altri, da che ogni leggier tocco della mano, o di checchè altro puo baſtare in tali circoſtanze favorevoli. Senza queſto la materia fuſa abbandonata a ſe ſteſſa nel rapprenderſi e dopo, tanto è lungi che contragga alcuna elettricità, che anzi perde quella qualunque che per ſorte aver poteſſe prima della fuſione, come le noſtre ſperienze ci aſſicurano. Nè ſia meraviglia: giacchè tutti i corpi coibenti per un forte grado di calore divengono conduttori; e i corpi reſinoſi in iſpecie lo ſono già quando ſi trovan molto rammolliti, e più allorche cominciano ad entrare in fuſione.

piegata è più debole. Così vedemmo già che ſe il conduttore atmosferico non ha la forza di alzare d'un grado il pendolino dall' elettrometro, movendo appena un ſottil pelo, ed anche meno di queſto, potrà tuttavia abilitare lo ſcudo non che a vibrar l'elettrometro a più gradi alto, ma a ſcagliare pur anche vivace ſcintilla (§ 2. e ſeq.). Ma ſe l'elettricità nel conduttore atmosferico ſarà più forte a ſegno di dare qualche ſcintilletta, di mandare l'elettrometro a 5.o,6 gradi, lo ſcudo che riceverá queſta elettricità, darà gli è vero una ſcintilla aſſai più forte, e l' elettrometro vibreraſſi al più alto punto eſ. gr. a 100 . 120 gradi. Ad ogni modo è viſibile che la condenſazione dell' elettricità ne in queſto é minore che nel p°. caſo, in cui venne aumentata ſi, ma non di 60 volte, la ragione è che al di la del maſſimo non ſi puo andare, cioè di quel grado a cui giunta l'elettricità ſi diſſipa da ſe ſteſſa aprendoſi il paſſagio per tutto. Dunque a miſura che la potenza elettrica, la quale ſi applica allo ſcudo poſato, è più vicina a tal ſommo grado, minor accreſcimento puo ricevere dall' apparecchio condenſatore. Ma che biſogno abbiamo noi allora di lui, e tutte le volte che l'elettricità è già ſenſibile e forte abbaſtanza ?

L'uſo a cui vien deſtinato è di ſottrarre, e raccolta ſopra di ſe ſufficiente doſe render ſenſibile quella, che è languida affatto e impercettibile, finchè rimane nel gran conduttore in pace (1.).

10. Quando dunque il conduttore vi dà già da ſe ſolo ſegni abbaſtanza diſtinti di elettricità, non accade ricorrere all' altro apparecchio. Dirò dippiù che il farlo puo produrre un grande inconveniente, ed è, che per poco che l'elettricità del conduttore ſia vigoroſa, a ſegno di dare qualche ſcintilla, avviene allora che facendogli toccare lo ſcudo, l'elettricità non ſi arreſti in lui ſolo, ma che paſſi in parte ad imprimerſi alla faccia reſinoſa cui copre; onde in ſeguito l'apparecchio prenda a fare le

funzioni di vero elettroforo: ciò che per le ragioni già dette (8.) ſi dee con ogni ſtudio evitare.

11. Per prevenire un tal inconveniente ho penſato di ſurrogare al piatto incroſtato di reſina, un piano che non foſſe vero e perfetto iſolante, aſſolutamente impermeabile al fluido elettrico; ma tale ſolamente che opponeſſe una diſcreta reſiſtenza al ſuo paſſaggio, come una laſtra di marmo aſciutta e politiſſima, un piattello di legno ſimilmente aſciutto ed arido, oppure incroſtato di geſſo, o meglio ancora inverniciato, una tela incerata ſecca e monda, od altro ſimile. Alla ſuperficie di tali corpi non avverrà d'ordinario che s'affigga l'elettricità potendo appiccata che ſia ſcorrere e trapaſſare per entro ad eſſi; o ſe pur talvolta ve ne rimaneſſe un pocolino, quaſi ſtagnante, ſia queſta paſſaggera, in brevi momenti ſvanita. Quindi è che un tal apparecchio inatto alle funzioni d'elettroforo non ce ne dará i fenomeni; ma per queſto appunto meglio ſervirà all' altr' uſo di condenſatore.

12. Soſtituendo coſi allo ſtrato reſinoſo o a qualſivoglia altro coibente perfetto un piano o ſtrato che ſia mezzo tra coibente e deferente, cioè un corpo iſolante molto imperfetto e inſieme imperfettiſſimo conduttore, quali ſono nelle diviſite circoſtanze gl' indicati corpi (prec.) non ſolamente ſi toglie o ſi fa minore il pericolo di qualche elettricità che poſſa imprimerſi e reſtar aderente alla ſuperficie del piatto, la quale renderebbe equivoche le ſperienze delicate; ma innoltre un notabile vantaggio da noi ſi ottiene, ed è che lo ſcudo poſato ſu tai piani non affatto iſolanti cava del conduttore, e ſi tira addoſſo maggior doſe di elettricità, che ſe poſato foſſe ſopra uno ſtrato reſinoſo, od altro perfetto coibente. E come detto già abbiamo (6.) che uno ſtrato reſinoſo quanto è men groſſo, tanto più abilita la lamina che gli è ſovrappoſta ad arricchirſi di elettricitá; coſi tale ſtrato

ridotto ad una ſemplice vernice, o intonaco di cera, l'una e l'altra giá men coibente della reſina, e infine ridotto a niente, ſoſtituendovi ſoltanto una ſuperficie poco deferente, come quella del marmo o del legno arido, ſomminiſtra alla lamina metallica la più favorevole poſitura che mai aver poſſa per raccogliere nel ſuo ſeno abondante elettricità.

13. Guardiamoci però nel voler iſchivare il troppo di coibenza di dare nel poco, accoſtandoci ai deferenti perfetti, o quaſi perfetti. Non biſogna perder di viſta, che la ſuperficie del piatto dee opporre una diſcreta reſiſtenza al trapaſſo del fluido elettrico, per rattenere una competente doſe di elettricità nello ſcudo addoſſatole (11.). Mà baſta che ciò faccia per un qualche piccioliſſimo tempo; d'uopo eſſendo non rare volte di tenervi confinata l'elettricità otto, dieci, e più minuti, quanti cioè ne impiega il conduttore atmosferico a raccoglier dall' aria, ed infondere in eſſo ſcudo tal copia di elettricità, che poſſa renderſi ſenſibile e coſpicua (3. D. 4.).

Dal che facilmente s'intende quanta attenzione pur convenga e nella ſcelta del corpo da ſurrogarſi allo ſtrato di reſina, e nella convenevole preparazione del medeſimo: la quale preparazione conſiſte generalmente in certo grado di eſſiccamento, che lo riduca allo ſtato di ſemicoibente nè più, nè meno. Ad ogni modo ſia meglio peccare per eccèſſo di coibenza, che per difetto; meglio prendere un piatto qualſivoglia incroſtato di reſina, che un deſco di legno nudo non aridiſſimo, una laſtra d'oſſo, od una di marmo comune non previamente riſcaldato al ſole o al fuoco: giacchè niun oſſo, e pochiſſimi tra i marmi ho trovato che valgano a tener confinata l'elettricità nello ſcudo che combaciano oltre ad un minuto o due al più, ſe abilitati non vengano da un convenevole riſcaldamento. Diſpoſti però che ſiano in tal modo, e ove ſingolarmente incontrata ſi ſia ottima qualità nel marmo, rieſcono a meraviglia, e ſorpaſſano ogni aſpettazione; onde ſoſterrò ſempre con ragione, che ſi fatti piani di legno, d'oſſo, di pietra, nudi come ſono, e ancora notabilmente deferenti, meritano tuttavia d'eſſere preferiti a un ordinario piatto d'elettroforo fornito del ſuo ſtrato reſinoſo.

14. Venendo ora più davvicino alla maniera, onde praticamente ſi puo ridurre il noſtro apparecchio alla maggior perfezione, per ritrarne il più gran vantaggio, dopo aver ricordato come conviene ſoprattutto che lo ſcudo s'adatti bene a combaciamento col piano ſottopoſto (3 lat. B. e 7.), ſoggiugnerò che per ottener ció nel miglior modo é bene d'appigliarſi ad una laſtra di marmo, e queſta inſieme alla lamina o ſcudo metallico ſpianare ben bene, lavorandole una ſopra l'altra, finchè ſian ridotte a tale perfetto combaciamento, che ne naſca ſenſibile coeſióne tre loro.

Il marmo poi coſi lavorato ſi eſponga per molti giorni al calore d'una ſtufa, con che eſpellendoſi l'umido di cui anche tali pietre ſono ſpeſſo inbevute, verrà il marmo condotto a queſto ſtato d'imperfettiſſimo conduttore, che è l'ottimo per le ſperienze di queſto genere (12. 13.); e ſi manterrà tale per un pezzo, ſol che non reſti lungamente eſpoſto al grand' umido: poichè per quell' umidòre che puo contrarre accidentalmente, e in poco tempo, non eſſendo che ſuperficiale, non verrà eſſo marmo a deteriorarſi notabilmente; e baſterà prima di ſperimentare eſporlo per alcuni minuti al ſole, o pur anche aſciugarlo con un pannolino caldo.

15. E quì giova avvertir di nuovo, che non tutti i marmi ſono egualmente buoni. In generale i più vecchi, e che da molto tempo ſono ſtati guardàti dal grand' umido rieſcono incomparabilmente meglio che quelli tratti di freſco dalla cava, e ſtati eſpoſti lungamente all' ingiurie dell' aria; onde queſt

ſolamente han biſogno dell' eſſiccamento nella ſtufa. Ma oltre di cio avvi ancora notabiliſſima differenza tra una ſpecie e l'altra di marmo: ne ho trovato di tali, che ſenza riſcaldarli ne tampoco aſciugarli fanno ſempre a meraviglia, e di tali altri, che anche con una tal preparazione non corriſpondono troppo bene; a meno che non ſi continui loro il' caldo durante il tempo dell' eſperienze. Sopra tutti finora ho trovato eccellente il bel marmo bianco di Carrara. Ciò non pertanto io non ſo abbaſtanza raccomandare di riſcaldare e queſto, e gl' altri marmi, almeno un poco innanzi adoperarli: con che vantaggian ſempre per eccellenti che ſiano ed eſſendo cattivi vengono a migliorarſi inſignemente, e ſi adagguagliarſi ai più buoni: anzi poſſo dire, per eſperienza che la maggior parte dei marmi di lor natura poco buoni, ove ſiano ben riſcaldati previamente, e in ſeguito ſi mantengano tiepidi tutto il tempo dell' eſperienza, prevalgono ſe non a tutti a molti dei migliori non punto riſcaldati.

16. A chi però ſembraſſe incomoda queſta preparazione (la quale per altro a che ſi riduce? Ad eſporre il piatto di marmo al ſole, od a preſentarlo per poco d'ore innanzi al fuoco d'un cammino, o al più tenerlo ſu d' uno ſcaldavivande ove ſia o cener calda o pochi carboni acceſi) io ſuggerirò il mezzo di diſpenſarſene: baſta di dare alla faccia piana del marmo una buona mano di vernice copal, da aſciugarſi quindi in una ſtufa ben calda o in un forno tantoche prenda un color d'ambra tirante al bruno. La vernice medeſima d'ambra ſarà ottima, ſiccome pure la lacca. Con ciò non ſolo i buoni, ma i cattivi marmi eziandio ſerviranno mirabilmente all' intento (chè ſi pure un gran vantaggio) e ſerviranno in ogni tempo ſenza previo riſcaldamento, o almeno ſenza continuarlo loro durante l'eſperienza.

17.

17. Appigliandoſi a queſto ſpediente della vernice ſi puo beniſſimo in luogo del piatto di marmo far ſervire una lamina di metallo eguale all' altra lamina o ſia ſcudo, e reſa perfettamente combaciante col lavorare, come ſi è detto (14.), i due piani un ſopra l'altro. Se la vernice ſi deſſe ad amendue le faccie combacianti, non ſarebbe male; ma baſterà anche il darla all' una o all' altra: in queſto caſo però una mano ſola di vernice, che ſaria più che ſufficiente, per la laſtra di marmo, forſe non baſteria per la lamina metallica, ma ce ne vorrebbe una ſeconda ed anche una terza mano.

18. Ma con ciò, mi ſi dirà, noi ſiam ricondotti ad un vero piatto d'elettroforo, giacchè l'intonaco di vernice tien qui luogo del ſottile ſtrato di reſina. Io non voglio negarlo; anzi dirò, d'aver provato che e il metallo, e il marmo, ſingolarmente così inverniciati, ſon tali, che l'elettricità vi ſi affigge facilmente, e non men facilmente vi ſi eccita per iſtrofinamento, talchè il ſolo ſtriſciare che faccia lo ſcudo ſulla ſuperficie inverniciata del piatto, o il percuoterla con qualche forza mentre ſi viene a poſar ſopra coteſto ſcudo, baſta perchè poi dia ſegni ſenſibili di elettricità allorchè ſi diſtacca. Talora anzi non è poſſibile d'impedire che queſto ſucceda, per quanto ſi procuri di poſar lo ſcudo pian piano, e di alzarlo ſenza punto ſtrofinare. Tal importuna elettricità però è deboliſſima e non ſi ſuſcita che nel caſo in cui il piatto verniciato ſi trova aſciugatiſſimo, e ancor tiepido dal ſole o dal fuoco. Si fatto aſciugamento e riſcaldamento adunque non ſolo non è neceſſario per le noſtre ſperienze quando adoperiamo un piano verniciato, com' è neceſſario quaſi ſempre ove s'adoperi il marmo nudo (13. 15. 16.), ma non è neppure molto proficuo da una parte; e dall' altra egli è aſſolutamente pregiudizièvole, per ciò che dando luogo ai fenomeni d'*elettroforo*, puo facilmente produrre equivoci ed incertezze (8.).

19. Qual vantaggio adunque, mi ſi dirà un' altra volta, nell' adoperare in luogo di un ordinario elettroforo, un piatto inverniciato? Altronde ſi è pur detto che vuol preferirſi un piatto nudo di marmo (11. e ſeq.). Il vantaggio del piatto verniciato ſopra l' un ordinario d'elettroforo è 1° che la vernice ſarà ſempre più ſottile di qualunque incroſtatura reſinoſa; 2°. che quella meglio che queſta puo laſciare la ſuperficie del piatto, ſia di marmo ſia di metallo, piana e liſcia in modo, che lo ſcudo vi s'adatti ancora quaſi a coeſione: due circoſtanze, quali veduto già abbiamo (6. 7. 14.) quanto influiſcano alla buona riuſcita delle ſperienze di cui ſi tratta. Riguardo al piatto nudo di marmo, egli é ben vero che queſto puo ſervire egualmente bene, e forſe meglio s'egli é d'ottima qualità, o allorche ſi tenga convenevolmente riſcaldato (13.); ma valutando bene le coſe, l'incomodo, cioè di tal preparazione, qualunque egli ſia (16.), e la difficoltà d'aver il marmo perfetto (15.), credo che convenga ancora l'eſpediente della vernice, che vi diſpenſa da tutto queſto (16.). Vi reſta è vero l'altro inconveniente di potervíſi per poco affiggere l'elettricità; ma oltrecchè anche il marmo perfettamente aſciutto, e molto più ſe caldo, non va eſente da tal incomoda diſpoſizione, egli non è poi tanto difficile di ciò ſcanſare adoperando le debite attenzioni; e l'accurato ſperimentatore non laſcerà di aſſicurarſi coi mezzi che giá ſi ſono indicati (8.) che non trovaſi neppur ombra di elettricità impreſſa alla faccia verniciata, quando imprende a fare col condenſatore delle ſperienze delicate.

20. Al piatto di marmo e di metallo inverniciato va di paro un piano qualunque coperto di buona tela incerata ſecca e monda, di taffetá cerato, di raſo o d'altro drappo di ſeta il quale più che è ſottile è meglio: dico, che queſti piani coſi veſtiti van di parò agl' altri verniciati, ſtante che non han biſogno che

d'avere cotal veſte ben aſciuta, e al più un pocolino riſcaldata prima di ſervirſene; anzi pure e la tela, e il taffetá, incerati non attraendo molto l'umido, non hanno d'ordinario neppur biſogno d'eſſere poſti al ſole o al fuoco innanzi farne uſo. Il ciamberlotto, il feltro, ed altri drappi di pelo ſon buoni anch' eſſi, ma men della ſeta; quei di lana, o di cotone, meno ancora; e i più infelici ſono quei di canape e di lino: ad ogni modo un buon aſciugamento, e un gentil calore continuato poſſono abilitare anche queſti, ſiccome pure abilitano la carta, il cuojo, il legno, l'avorio, e gl' altri oſſi, tutti inſomma i corpi che ſono da ſeſteſſi imperfettiſſimi conduttori, anzi non conduttori, ma troppo bibaci dell' umido, cui perciò convien eſpellere fino a un certo ſegno.

21. Dico *fino a un certo ſegno:* perchè un troppo grande iſolamento è pregiudizievole anzichè no, come ſi è già accennato (6. 12.), e come ſi farà più chiaramente vedere nell 2ª parte di queſta memoria. Or dunque ſe i detti corpi vengano ſpogliati affatto d'umido, poſti per eſempio a ſeccare nel forno, in tal caſo ſiccome diverranno veri e perfetti coibenti a par delle reſine; così non ſerviranno più al noſtro intento, a men che non ſian ridotti ad uno ſtrato ſottile, e queſto ſtrato applicato ad un conduttore (ivi) in modo che ne riſulti un vero piatto d'elettroforo.

22. Non laſcerò da ultimo di dire, che ſi puo rendere l'apparecchio ancora più ſemplice, ſe ſi applichi ſia l'intonaco di vernice, ſia la veſte d'incerato, ſia il taffetà od altro velo di ſata, ſia infine qualunque materia ſemicoibente, alla faccia inferiore dello ſcudo, in luogo di coprirne il *piatto*; il quale in queſto caſo diventa inutile, ſervendo allora in ſua vece un piano qualunque egli ſia, un tavolo di legno o di marmo, anche non ben aſciutti, una laſtra di metallo, un libro, od altro conduttore, buono o

cattivo che ſia, ſol che vi ſi poſſa applicare convenientemente la faccia veſtita dello ſcudo.

E' in vero altro più non ſi ricerca per la buona riuſcita delle ſperienze, ſe non che l'elettricità, che tende a paſſare dall' uno all' altro dei piani combaciantiſi, incontri ſull' una delle ſuperficie tale reſiſtenza, che valga a trattenerla, come ſi è già accennato (11); e ſi farà chiaro nella ſteſſa ſeconda parte; dove al dippiù moſtreraſſi, come a tal effetto baſti anche una picciola reſiſtenza. Ciò poſto, che lo ſtrato ſottile coibente o quaſi coibente tenga al piano di ſotto, o a quel di ſopra, egli è lo ſteſſo: quello che importa è che ſi combacino bene (7); la qual coſa non è ſi facile ottenere allorchè ſi poſa lo ſcudo ſu d'un tavolo, od altro piano non preparato a bella poſta. Egli é ſolo per queſta ragione, per ottenere cioè un più eſatto combaciamento, che io dò la preferenza a due piani lavorati un ſopra l'altro intonacandoli quindi od amendue, o uno ſolo, qual più mi piace (14. 17.). Del reſto la comodità d'avere per tutto l'apparato una ſola lamina di metallo inverniciata da un lato, o coperta di taffetà, e dall' altro guernita di trè cordoncini di ſeta, fa che io me ne ſerva più comunemente: e la riuſcita ſe non agguaglia per avventura quella dell' altro apparechio compoſto dei due piani lavorati un ſupra l'altro, è tale però che baſta d'ordinario all' intento.

23. Fin qui noi abbiamo conſiderato l'utile che ſi puo ritrarre dal noſtro apparecchio condenſatore applicato ai conduttori per eſplorare l'elettricità atmosferica, allorchè è debole affatto ed impercettible *. Queſto però, a cui vien deſtinato prin-

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* A queſto propoſito non debbo ommettere, che nè pochi giorni in cui m'applicai a ſpiare l'elettricità atmosferica col ſoccorſo del *condenſatore*, non ſon rimaſto ſenza buon frutto raccorne. Il Sig. CANTON, ed altri aſſicuravano di aver ottenuto dall'

principalmente, non è il ſolo uſo che far ſe ne poſſa, nè il ſolo vantaggio che ci procura: ſerve altreſi molto per l'elettricità artificiale, a diſcoprirla cioè ove per altra via non ſi manifeſterebbe, o a renderne i ſegni aſſai più coſpicui. Molti ſono i caſi, in cui, l'elettricità, che è nulla in apparenza, o molto dubbia, vi ſi renderà chiara e ſenſibiliſſima coll' ajuto di tal apparecchio: ne andrò accennando per modo d'eſempio alcuni.

24. 1°. Una boccia di Leyden caricata, e quindi addotta alla ſcarica coll' applicarvi tre, o quattro volte l'arco conduttore, e con replicati toccamenti della mano, vi ſembra omai ſpogliata affatto della ſua elettricità. Ma che? Toccate coll' uncino di tal boccetta la lamina metallica poſata convenevolmente (cioè ſopra qualunque piano, s'ella é ben inverniciata nella faccia inferiore, o veſtita di taffetà, &c. oppur s'è nuda ſopra ſottile ſtrato reſinoſo, o ſu d'un incerato, o ſu drappo di ſeta, o ſopra tavolo di legno inverniciato, o ſopra laſtra di marmo ben aſciutto) e toſto alzata cotal lamina o ſcudo ne avrete ſegni elettrici ſenſibiliſſimi: dal che concluderete che l'elettricità della boccetta non era già tutta ſpenta, come appariva. Che ſe queſta aveſſe una carica ſenſibile a ſegno di

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dall' apparato atmosferico de ſegni elettrici più vivi dell' ordinario in tempo di qualche aurora boreale; ma molti de fiſici non ſono perſuaſi ancora che l'elettricità influiſca in queſte meteore, e alcuni lo negano formalmente. Io ſteſſo ne dubitai moltiſſimo: ora però parmi la coſa certa; e poſſo dire d'aver veduto e toccato con mano. In quella belliſſima aurora comparſa nella notte dei 28 ai 29 Luglio dell' anno 1780, quando ſalendo a poco a poco dall' orizzonte fu aſceſa tra la 4. e le 5. italiane allo zenit, ſpargendo tutt' all' intorno un vaghiſſimo lume roſſigno, il cielo altronde eſſendo ſereno e ventoſo, ſi ottennero col favore dell' apparechio condenſatore da un conduttore atmosferico ordinario molte belle ſcintillette chiare e crepitanti: quando in tutti gl' altri tempi ſereni, e in ogni ora del giorno e della notte dall' iſteſſo conduttore, e coll' ajuto dell' iſteſſo cendenſatore o non ottienſi ſcintilla o minutiſſima ſoltanto; e ciò perchè quel conduttore atmosferico non è nè alto molto, nè molto ben ſituato.

attrarre un legger filo, in tal caſo lo ſcudo toccato dall' uncino anche per un ſol momento, e quindi alzato vibrerà vivace ſcintilla. Ripoſto quello, e ritoccato coll' iſteſſo uncino della boccia, e rialzato di nuovo, ne otterrete una ſeconda ſcintilla, nulla o poco men vivace della prima; e un tal giuoco potraſſi continuare per molte volte con pari diletto e meraviglia.

Coteſto artificio di produr ſcintilla, e replicate, con una boccetta, che non ha carica ſufficiente per farlo da ſê ſola, vi appreſta una grande comodità per varie ſperienze dilettevoli, come quelle della mia piſtola, e della lucerna ad aria infiammabile, maſſimemente trovandovi provveduto d'una di quelle boccette preparate alla maniera del Sig. TIBERIO CAVALLO *, le quali ſi poſſono portare cariche in taſca molto tempo. Queſte poichè conſervano una carica ſenſibile alcuni giorni, ne conſerveranno una inſenſibile per ſettimane, e meſi: inſenſibile, dico, ſenza l'ajuto del noſtro apparecchio condenſatore; ma con queſto ſenſibiliſſima, e più che ſufficiente all' uopo di accender la piſtola, &c.

25. 2°. Avete una macchina elettrica meſchina, così mal in ordine, e in tali circoſtanze sfavorevoli d'umido &c. che non potete trarre la più piccola ſcintilla dal conduttore, il quale appena attrae un leggeriſſimo filo, o non giugne neppur a tanto. Or via fate toccare a tal conduttore inerte il noſtro apparecchio, oſſia lo ſcudo poſato come conviene, e laſciate che il toccamento duri per qualche minuto, tenendo ſempre in azione la macchina, e vi riuſcirà di ottenere col ſolito giuoco di ſtaccare lo ſcudo dal ſottopoſto piano, una buona ſcintilla, e ogn' altro ſegno vivace.

26. 3°. Sia pur la macchina buona, e agiſca a dovere; ma il conduttore troviſi così male iſolato, che l'elettricità non vi ſi

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* Vedi il ſuo trattato di elettricità.

poſſa accumulare a ſegno di dar ſcintilla, e neppure di attrarre un filo: come quando l'iſteſſo conduttore tocca al muro della ſtanza, o quando una catena pende da eſſo ſopra un tavolo, e fin ſopra il pavimento della ſtanza. In ſimil caſo crederete che l'elettricità per quelle comunicazioni ſi diſperda intieramente, ma cercando più oltre, ricorrendo cioè al condenſatore, troverete che un poco ſe ne trattiene ad ogni momento nel conduttorre tuttochè non iſolato, e tanto che durando l'azione della macchina qualche tempo, i molti pochi raccolti inſieme nello ſcudo, per la vantaggioſa diſpoſizione ch' egli ha di tirar ſopra di ſe l'elettricità (2.) fanno ch' il medeſimo ſia poi in iſtato di dar ſegni abbaſtanza forti.

27. 4°. L'ordinaria maniera di ſtrofinare alcuni corpi, e quindi preſentarli ad un elettrometro, onde vedere ſe per tal mezzo abbiano o no contratto qualche elettricità, e in molti caſi inſufficiente, di modo che ſovente ſi crede che ſia nulla, ſol perchè deboliſſima. Si trae dunque un gran vantaggio ſtrofinando corpi dubbi collo ſcudo o lamina metallica del noſtro apparecchio, chè in queſto caſo deve eſſer nuda, poi levatala in alto iſolata interrogando lei medeſima, la quale darà ſegni abbaſtanza ſenſibili per qualunque picciola ad inſenſibile elettricità eccitata nel corpo, contro cui ſi è ſtrofinata, e dinoterà quale ſpecie di elettricità quello abbia contratta, giacchè ſi ſa che debbe eſſere nei due contraria. Anche il Sig. CAVALLO ſi ſerviva di queſto mezzo per iſcoprire l'elettricità in molti corpi *. Ma ve n' è uno a certi riguardi migliore, che certamente nè egli nè altri, ch' io ſappia, han conoſciuto. Quando il corpo, di cui ſi vuol provare la virtù, non è tale che vi ſi poſſa addattare in piano la lamina metallica per dimenarla ſopra ſtrofinando, ſi puo fare così: poſata la lamina ſopra il ſolito piano

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* Vedi il ſuo trattato, cap. VI. p. iv.

ſemicoibente, ſi ſtrofini eſſa, o meglio ſi percota a vari colpi col corpo in queſtione; il che fatto ſi levi la lamina, e ſi oſſervi ſe è elettrizzata: lo ſarà certo nel caſo che vi ſiate ſervito a percuoterla di una ſtriſcia di cuojo, d'una corda, d'un pezzo di panno, di feltro, o ſimili cattivi conduttori; e lo ſarà aſſai più che ſe l'aveſte sferzata o ſtrofinata par egual maniera coi medeſimi corpi ſtando eſſa lamina metallica iſolata. In ſomma coll' uno o coll' altro degl' indicati mezzi voi otterrete elettricità da corpi che non avreſte mai creduto che godeſſero di queſta virtù, anche da corpi non ſecchi, da tutti infine eccetto ſolo i metalli e i carboni: dirò dippiù, ch' io ne ho ottenuto qualche volta ſtrofinando la lamina metallica colla mano nuda.

28. 5°. Si è cercato ſe il calore, l'evaporazione, le fermentazioni, &c. producano qualche grado di elettricità, oſſia cagionino qualche alterazione alla doſe naturale del fluido elettrico nei corpi che ſubiſcono coteſta azione, e in quelli che ne ſono in contatto. La ricerca era di grande importanza per fiſſar pure qualche idea ſull' origine dell' elettricità naturale, oſſia atmosferica. Io ſo di molti che hanno tentato ſpecialmente ſull' evaporazione delle ſperienze invano, anche hanno infine rinunciato alla ſperanza di ottenere per tal mezzo ſegni elettrici; nè ſo d'alcuno che ſia ancor giunto ad ottenerli. Le mie proprie ſperienze non avean avuto miglior ſucceſſo; ad ogni modo ben lungi di rinunciare ad ogni ſperanza, le andava ſempre più nodrendo. Da gran tempo io aveva imaginato che le diſſoluzioni, le effervefcenze, le volatilizzazioni, &c. ſconvolgendo le minime particelle, e forma e poſizione mutandone, doveano coll' alterazione delle forze mutue di eſſe particelle aumentare o diminuire le riſpettive capacità dei corpi ſottopoſti a que moti inteſtini, e conſequentemente occaſionare dove condenſazione, dove rarefazione del fluido elettrico: ne era coſi perſuaſo, che

non ſapevo darmi pace che l'elettricità non ſi manifeſtaſſe per alcuno di tai proceſſi; di tal mancanza di ſegni pertanto io ne accagionavo parte alla debolezza dell' elettricità che per tal modo ſi eccitava, parte alla diſſipazione di eſſa prodotta dai vapori medeſimi che ſi ſollevano durante il proceſſo, e diſtruggono quaſi intieramente l'iſolamento: mi compiaceva però ſempre a penſare, che l'avrei un giorno potuta ſcoprire coteſta elettricità fugace, moltiplicando le ſperienze, e mettendovi più di attenzione e di accuratezza*. Due anni ſono allorchè fui paſſo paſſo condotto alla maniera di condenſare a un ſegno ſi grande l'elettricità coll' apparecchio qui deſcritto, i miei penſieri ſi rivolſero nuovamente all' oggetto delle antiche mie ricerche, e concepj molto più fondata ſperanza di poter iſcoprire qualche coſa, e già mi proponeva di applicarmi, a tali ſperienze, quaſi preſagendo la riuſcita; ma varj accidenti le ritardarono fino al Marzo e Aprile di queſt' anno, in cui intrapreſe avendole a Parigi in compagnia di alcuni membri dell' Accademia R. delle Scienze, mi riuſcì finalmente di ottenere ſegni non dubbj di elettricità (che dico ſegni non dubbj?) fin la ſcintilla elettrica dall' evaporazione dell' acqua, dalla ſemplice combuſtione dei carboni, e da varie efferveſcenze, come quelle che producono l'aria infiammabile, l'aria fiſſa e l'aria nitroſa.

29. Terminerò la prima parte di queſta memoria coll' dire, che oltre gli accennati vantaggi, ed altri del medeſimo genere, che ne procura il noſtro condenſatore conſiderato ſemplicemente come iſtromento, atto ad ingrandire i ſegni dell' elettricità; le varie ſperienze che poſſono farſi con eſſo ſpargono eziandio molto

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* Tutti queſti miei penſieri ſono eſpoſti in una diſſertazione latina ſtampata l'anno 1769, che ha per titolo, *De vi attractiva ignis electrici, ac phenomenis inde pendentibus, ad* JOHANNEM BAPTISTAM BECCARIAM, &c.

lume ſulla teoria elettrica, per quella parte maſſimamente che riguarda l'azione delle atmosfere: lo che andiamo a vedere nella parte 2ª.

PARTE SECONDA.

IN qual maniera un conduttore accoſtandoſi a un altro ſotto certe condizione, ſi truovi in iſtato di ricevere una ſtraordinaria quantità di elettricità.

30. Le ſperienze riportate nella prima parte di queſta memoria ci hanno abbaſtanza moſtrato come una lamina metallica o qualſivoglia piano conduttore, cui ſoglio appellare *ſcudo*, applicato ad un altro piano, il quale opponga, o per la qualità ſua di cattivo conduttore, o per l'interpoſizione di un ſottile ſtrato coibente, una certa non grande reſiſtenza alla trasfuſione dell' elettricità, come diſſi, tale ſcudo in ſiffatta poſizione atto ſia a tirare ſopra di ſè e raccorre nel ſuo ſeno maggiore copia di elettricità, che ſe ſi trovaſſe in qualſivoglia modo perfettamente iſolato. Abbiam veduto come facendolo toccare all' uncino di una boccia di Leyden, al conduttore di una macchina elettrica, o a quello dell' elettricità atmosferica, infine a qualunque potenza o ſorgente elettrica, anche quando l'elettricità è deboliſſima e affatto impercettibile, pur gli ſe ne comunica tanto da poter manifeſtarſi quindi con ſegni molto vivaci, toſto che ſi leva eſſo ſcudo in alto. Or qui intrapreudiamo di ſpiegare un tal fenomeno: e la ſpiegazione medeſima ſervirà più ch' altra coſa a facilitare la pratica delle ſperienze di queſto genere.

31. Adunque il tutto ſi riduce a queſto: che la lamina o ſcudo ha molto e molto maggiore capacità nel 1° caſo, quando cioè poſa ſul piano avente le condizioni indicate (præc. e 11. 12. 22.), che nel 2°, in cui tienſi eſ. gr. in alto ſoſpeſo per i ſuoi cordoncini di ſeta, o per un manico iſolante, oppur che poſa ſopra un groſſo ſtrato coibente, o ſopra un piatto iſolato.

Per dilucidare queſto punto eſſenziale, prendiam le coſe da più lontano.

32. Non vi vuol molto a comprendere, che' ivi è maggiore capacità, dove una data quantità di elettricità ſorge a minor intenſità, o che è lo ſteſſo, quanto maggior doſe di elettricità è richieſta a portare l'azione a un dato grado d'intenſità; e *viceverſa:* a dir breve, la capacità e azione, o tenſione elettrica ſono in ragione inverſa.

Farò qui oſſervare ſul principio, ch' io dinoto col termine di tenſione (che volentieri ſoſtituiſco a quello d'intenſità) lo sforzo che fa ciaſcun punto del corpo elettrizzato per disfarſi della ſua elettricità, e communicarla ad altri corpi: al quale sforzo corriſpondono generalmente in energia i ſegni di attrazione, ripulſione, &c. e particolarmente il grado a cui vien teſo l'elettrometro.

33. Ciò che abbiam detto comprenderſi facilmente che la tenſione debb' eſſere in ragione inverſa delle capacità, ci viene poi moſtrato nella maniera più chiara dall' eſperienza. Siano due verghe metalliche, una lunga 1 piede e l'altra 5. di groſſezza equali. S'infonda alla prima tanto di elettricità, che giunga a vibrare un elettrometro anneſſo a 60 gradi: ſe in queſto ſtato ſi farà toccare quella all' altra verga, l'elettricità compartendoſi equabilmente ad ambedue, diminuirà di tenſione tanto appunto, quanto la capacità ſi truova ora accreſciuta, cioè 6 volte: locchè ci farà vedere l'elettrometro, ſmontando dai 60, ai

10 gradi. Così ſe l'iſteſſa quantità di elettricità veniſſe a diffonderſi in un conduttore 60 volte più capace, non rimarrebbe che $\frac{1}{60}$ della primiera tenſione, cioè un grado ſolo: come *vice-verſa* la tenſione di 1 ſol grado di coteſto gran conduttore, o d'altro qualunque, ſalirebbe a 60, ove la di lui elettricità veniſſe a raccorſi e condenſarſi in una capacità 60 volte minore.

34. Or non ſolo conduttori di mole e maſſa diverſi hanno diverſa capacità; ma anche l'iſteſſo conduttore puo averne una maggiore o minore, ſecondo varie circoſtanze; alcune delle quali non ſono per anco ſtate conſiderate, come ſi conviene. E ſtato oſſervato che l'iſteſſo conduttore acquiſta o perde in capacità, a miſura che ſi aggrandiſce, o ſi riſtringe di ſuperficie; ſecondo che una catena metallica eſ. gr. ſi diſpiega in lungo, o ſi ammucchia, ſecondo che vari cilindri contenuti un nell' altro, come quelli d'un cannochiale ſi traggon fuori, o ſi fanno rientrare, &c. Quindi ſi è concluſo generalmente che la capacità non è in ragion della maſſa, ma bene in ragion della ſuperficie del conduttore: come FRANKLIN ha dimoſtrato appunto coll' indicato ſperimento della catena.

35. Queſta concluſione e giuſta; ma non comprende ancor tutto, perocchè anche con ſuperficie egualmente grandi ſi ha maggiore o minore capacità, ſe ſiano i conduttori diverſamente conformati. Eſſa ſi troverà maggiore di molto in quel conduttore che avrà più lunghezza comunque ſia d'altrettanto men groſſo, coſicchè la quantità della ſuperficie rimanga eguale: come WATSON ed altri aveano già oſſervato, e come io mi luſingo d'aver poſto in miglior lume nella mia memoria ſulla capacità de conduttori ſemplici *, nella quale dimoſtro il grande vantaggio di un conduttore coſtrutto di molte verghe di legno coperte di foglia metallica, e collocate in lungo punta a punta,

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* Fu pubblicata queſta memoria in un opera periodica di Milano intitolata *Opuſcoli Scelti*, per l'anno 1778, e nel Giornale dell' Ab. ROZIER l'anno ſequente.

ſopra gl' ordinarj conduttori aſſai più groſſi e meno lunghi. Se l'iſteſſo conduttore colla groſſezza e lunghezza medeſima non ſia diritto, ma aſſai curvo, e molto più ſe eſſendo eſ. gr. un fil di ferro, abbia molti torcimenti, o ſi ripieghi indietro, avrà minore capacità; così pure l'avranno minore le indicate verghette, ſe invece d'eſſer collocate punta a punta in linea retta, lo ſiano ad angolo, e peggio ſe s'accoſtino parallele.

Le ſperienze ed oſſervazioni da me rapportate in quello ſcritto, ed infinite altre, maſſimamente quelle intorno al così detto *pozzo elettrico*, concorrono tutte a provare, che la capacità è in raggione non delle ſuperficie qualunque eſſe ſieno, ma delle *ſuperficie libere dall' azione delle atmoſfere omologhe*: nella quale rettificata propoſizione converranno tutti quelli, che ſi faranno a conſiderare i principali fenomeni delle atmoſfere elettriche.

36. Ma v' è dippiù ancora: e queſto è propriamente che fa al noſtro caſo. L'iſteſſo conduttore ritenendo la ſteſſa ſuperficie, e la forma ſua non mutata, acquiſta maggiore capacità allorachè in luogo di rimanere iſolato ſolitariamente, ſi affaccia a un altro conduttore non iſolato, e l'acquiſta tanto ſempre maggiore quanto vi ſi affaccia più davvicino, e quanto le ſuperficie che ſi preſentano un l'altro ſono più larghe. Io chiamo quel conduttore iſolato che ne ha un altro di fronte (ſia queſto non iſolato, come nel caſo noſtro, ſia anche iſolato, elettrizzato o no), lo chiamo *conduttore conjugato*; e già io aveva promeſſo nella mentovata diſſertazione, trattato avendo della capacità dè *conduttori ſemplici*, o *ſolitari*, di trattare in ſeguito di quella de *conduttori conjugati*.

37. Tale circoſtanza, che accreſce prodigioſamente la naturale capacità di un conduttore, quella è ſopra tutto, a cui non truovo che ſi ſia fatta ancora la debita attenzione; molto meno che alcuno ne abbia tratto quei vantaggi, che dall' applicazione

facilmente ne derivano. Ma veniamo a quelle ſperienze più ſemplici, che ci mettono ſott' occhio queſta accreſciuta capacità.

Prendo un diſco di metallo, il ſolito ſcudo d'elettroforo per eſempio, e tenendolo in alto iſolato lo elettrizzo a una data forza, quanto baſta, ſupponiamo, a fare che un elettrometro anneſſo ſi tenda a 60 gradi; calando indi eſſo diſco gradatamente verſo un tavolo od altro piano deferente, ecco che decade l'elettrometro a 50, 40, 30 gradi. Non crediate perciò che ſia ſcemata a queſto punto la quantità d'elettricità che il diſco poſſiede, la quale anzi, purchè quello non ſia giunto a tale vicinanza dell' altro piano deferente da dar luogo alla trasfuſione collo ſcoccare di qualche ſcintilla, ſi ſarà mantenuta nell' intierezza ſua, quanto almeno la lunghezza del tempo, lo ſtato dell' aria e dell' iſolamento lo permette. Onde dunque tale e tanto abbaſſamento di *tenſione*? Non altronde che dall' accreſciuta capacità del diſco, or non più ſolitario, ma *conjugato*. In prova di che ſe ſi ſollevi di nuovo gradatamente, riſalirà il ſuo elettrometro a 40, 50, e fin preſſo ai 60 gradi di prima (riſalirebbe a 60 giuſto, ſe ſi poteſſe impedire affatto il diſſipamento nell' aria, e lungo gl' iſolatori non mai perfetti abbaſtanza); a miſura cioè che allontanandoſi dall' altro piano deferente ritorna il diſco a quella più anguſta capacità, che gli compete quand' è ſolitario.

38. La ragione di un tal fenomeno ſi deduce facilmente dall' azione delle *atmosfere elettriche*. Quella del diſco, che or ſuppongo elettrico *per eccеſſo* ſi fa ſentire al tavolo, od altro qualſivoglia conduttore, a cui ſi affaccia in guiſa che il fuoco di queſto, giuſta le note leggi, ritirandoſi ſi dirada nelle parti più vicine al diſco ſovraſtante, e tanto più ſi dirada, quanto eſſo diſco elettrico ſi va accoſtando. Se l'elettricità di queſto è *per difetto*, il fuoco del tavolo o piano inferiore qualunque ſia,

accorre e ſi addenſa verſo la ſuperficie medeſima, che guarda il diſco, e che ne ſente più davvicino l'azione. Inſomma le parti immerſe nella sfera di attività del diſco contraggono un elettricità *contraria*, elettricità che puo dirſi *accidentale*, e che portando in certo modo un *compenſo* a quella *reale* del diſco medeſimo, ne diminuiſce la *tenſione*, come appunto ci dimoſtra l'abbaſſamento dell' elettrometro (præc.)

39. Due altre ſperienze porranno in maggior lume queſta azione reciproca delle atmosfere elettriche, mercè di cui ora s'infievoliſcono, ora ſi rinforzano mutuamente le *tenſioni* oſſia azioni elettriche di due corpi col ſolo avvicinarſi l'uno all' altro, ritenendo ciaſcuno nè più nè meno la ſua doſe di elettricità.

Cominciamo da quelle che ſi rinforzano. Queſte ſono le *atmosfere omologhe*. Siano pertanto due piani conduttori, due diſchi, elettrizzati o per ecceſſo amendue, o amendue per difetto. Si affaccino queſti, e ſi vadano gradatamente avvicinando: vedraſſi che influiſcono l'uno ſull' altro in modo, che la *tenſione* elettrica s'accreſce in amendue a proporzione del più grande avvicinamento, e della quantità di ſuperficie che ſi preſentano: ciò, dico, vedraſſi dal maggiore innalzamento dè riſpettivi elettrometri, e dalla ſcintilla, che eſplorando l'uno o l'altro di quei diſchi ſcoccherà a maggior diſtanza, che ſe ciaſcuno foſſe rimaſto con tutta la ſua elettricità *ſolitario*. In quello ſtato adunque di avvicinamento egli è chiaro, che ciaſcuno de due conduttori *conjugati* ha una minore capacità; giacchè a proporzione che ſono già *attuati* a un più altro grado di elettricità, lor reſta meno per giugnere al ſommo, o a parlar più giuſto, maggiore è la reſiſtenza che oppongono ad un ulteriore carica, conformemente a quanto oſſervato già abbiamo (33.) che la tenſione eſprime lo sforzo, onde un corpo tende a disfarſi dell' elettricità, e a comunicarla altrui. Così una boccia di

Leyden carica a un grado un poco maggiore di quello dei diſchi *ſolitari*, la quale per conſeguenza *darebbe* loro in tale ſtato, *riceverà* all' incontro da eſſi quando eſſendo *conjugati* vi prevale la *tenſione*: ritornando queſti ſolitari, cederanno un altra volta alla boccetta, &c.

Or anche ſi comprende quello, che abbiamo fatto più ſopra oſſervare (36.), onde ſia cioè che un filo metallico ripiegato, o molte verghe poſte allato e vicine le une alle altre, abbiano minore capacità che diſpoſte l'une all' altre in una linea retta; perchè con ſuperficie equali un conduttore corto e groſſo abbia meno capacità d'un lungo e ſottile; perchè infine la capacità ſia in ragione delle ſuperficie *libere*, o meno *attuate* dall' influſſo delle *atmosfere omologhe*.

40. Siano ora i medeſimi diſchi della ſperienza precedente ambi ellettrizzati, ma uno *per eceſſo* l'altro *per difetto*, ben ſi vede che ne ſeguiranno effetti contrarj: cioè l'influenza vicendevole delle atmosfere, per cui l'uno è *attuato* dall' altro, produrrà un *compenſo* od *equilibrio accidentale*, onde diminuiraſſi la *tenſione* in amendue, cadrà l'elettrometro, &c. Allora io dico che trovaſi accreſciuta in ciaſcuno de due diſchi la capacità, inquantochè opporrà ciaſcuno minor reſiſtenza ad un ulteriore carica dell' elettricità che già poſſiede, e gliene rimarrà dippiù a prendere per giugnere a un dato grado di *tenſione*. Così una boccetta di Leyden carica dell' iſteſſa ſpecie d' elettricità d'uno di queſti diſchi, e all' iſteſſo grado ed anche al diſotto, potrebbe tuttavia aggiungere all' elettricità di quello, quando, trovandoſi *conjugato*, la ſua *tenſione* è indebolita dall' atmosfera elettrica contraria del diſco compagno; ma rimoſſo quello da queſto, e divenuta in lui le *tenſione prevalente*, darebbe egli della ſua elettricità alla boccetta, &c.

41. Non reſta più ora che fare un applicazione di queſt' ultima ſperienza a quelle riportate di ſopra (38.), in cui il diſco

elettrizzato ſi affaccia a un piano conduttore non iſolato. S'egli è vero, come ſuppoſto abbiamo che queſto nella parte più vicina a detto diſco elettrico, per l'azione della di lui atmosfera, ſi compone ad un elettricità contraria, vale a dire che il fuoco ivi ſi dirada qualor l'incombente elettricità ſia *in più*, o vi ſi condenſa qualor ſia *in meno* (39.), dovrà dunque naſcere l'iſteſſo *equilibrio* accidentale, l'iſteſſo *compenſo*, e alleviamento alla *tenſione* elettrica del diſco, lo ſteſſo abbatimento dell' elettrometro, come appunto ſi oſſerva (38.); quindi l'accreſciuta capacità di eſſo diſco; quindi la maggior doſe di elettricità che potrà ricevere (prec.) &c.

42. La coſa è già baſtantemente chiara, ma ſi renderà ancora più manifeſta, e toccheraſſi con mano, ſe ſi venga ad iſolare il piano conduttore (ſupponiam che queſto ſia parimenti un diſco metallico, che chiameremo diſco inferiore) affacciato già al diſco elettrico, e dopo ſi allontanino un dall' altro; giacchè allora compariranno realmente in eſſo piano o diſco inferiore i ſegni dell' elettricità contraria da eſſo lui acquiſtata allorchè non era iſolato, e trovavaſi immerſo nell' atmosfera del diſco ſuperiore. Coteſto diſco ſuperiore poi, il quale intantochè ſi allontana, ricupera la *tenſione*, che l'avvicinamento gli avea fatto perdere, la perderà di nuovo a miſura che ſi accoſterà un altra volta al diſco inferiore, e la farà perdere a lui medeſimo, in virtù dell' azione reciproca delle contrarie elettricità (41.) a indicare le quali vicende è opportuno che trovisi un elettrometro anneſſo a ciuſcuno de diſchi; poichè il linguaggio dell' elettrometro è il più ſignificante di tutti, e ardiſco dire ch' eſſo ſolo vi dà la ſpiegazione di tutti i fenomeni riportati in queſto ſcritto, e d'infiniti altri analoghi.

43. Che ſe il diſco inferiore ſi truovi iſolato, al primo affacciarvi il diſco ſuperiore elettrizzato, e iſolato rimanga tutto il tempo che queſto vi ſta ſopra, in tal caſo venendo *attuato* dalla

di lui atmosfera, acquiſterà quella che chiamo elettricità *omologa accidentale*, cioè una *tenſione* od azione elettrica, con cui fa sforzo di conſequire l'elettricità contraria; il che non venendogli dato di effettuare, per l'iſolamento in cui ſi truova, non potrà neppur *compenſare* nel dovuto modo l'elettricità del diſco incombente, nè quindi diminuire in lui la *tenſione* notabilmente, dimodochè l'elettrometro appena farà cenno di abbaſſarſi (il qual picciolo abbaſſamento ſi deve a quel poco di fuoco, che per l'azione dell' atmoſfera elettrica puo moverſi nella ſpeſſezza del qualunque diſco inferiore, o lungo i ſuoi ſoſtegni iſolanti non mai perfetti abbaſtanza); e per conſeguenza non acquiſterà il diſco ſuperiore maggiore capacità, onde poter prendere maggior doſe di elettricità. Ma bene l'acquiſterà, ſe un momento ſi venga a toccare il diſco inferiore, onde diſtruggere in eſſo l'elettricità *accidentale omologa*, che vuol dire fargli prendere la *reale contraria*.

44. Se il diſco inferiore, non che trovarſi iſolato, ſia egli medeſimo iſolante, ſuccederà lo ſteſſo, cioè non potrà diminuire la *tenſione* elettrica nè quindi aumentare la *capacità* del diſco ſuperiore accoſtatogli comunque. Non così però ſe cotal diſco iſolante ſarà ſemplicemente un ſottile ſtrato che copra un conduttore; mercecchè queſto piano conduttore che trovaſi poco ſotto, e in cui può moverſi liberamente il fuoco, farà eſſo il giuoco di *compenſare* l'elettricità del diſco ſuperiore; e lo ſtrato iſolante interpoſto diminuirà ſoltanto l'azion mutua delle atmoſfere elettriche, in ragione della maggior diſtanza che pone tra l'uno e l' altro conduttore.

45. La *tenſione* oſſia azione elettrica del diſco, la quale, come abiam veduto (38. 42.) va diminuendoſi a miſura ch' egli ſi affaccia più davvicino ad un piano deferente non iſolato, è por-

tata a un tale decadimento quando ſi arriva quaſi al contatto, il *compenſo* od *equilibrio accidentale* eſſendo allora quaſi perfetto, che dove l'elettrometro era teſo a 60, 80, 100 gradi, ſi vedrà or diſceſo a 1 grado ſolo, ed anche meno. Quindi ſe il piano o diſco inferiore opponga ſolo una picciola reſiſtenza al trapaſſo dell' elettricità, o per l'interpoſizione d'un ſottile ſtrato coibente, o per la natura ſua propria d'imperfetto conduttore, qual è il marmo aſciutto, il legno ſecco, &c. tale picciola reſiſtenza congiunta a quella della diſtanza comunque piccioliſſima non potrà eſſere ſuperata da tale deboliſſima *tenſione* del diſco elettrico; il quale perciò non iſcaglierà ſcintilla al piano (ſalvo che forſe dagl' orli non ben ritondati, e nel caſo che poſſieda una gran copia di elettricità); anzi conſerverà tutta o quaſi tutta la ſua elettricità, dimodoche rialzandolo, il ſuo elettrometro aſcenderà quaſi al grado di prima. Più: potrà il diſco ſenza gran detrimento della ſua elettricità giugnere fino al contatto del piano imperfetto conduttore, e reſtarvi qualche tempo applicato: nel quale contatto la *tenſione* elettrica trovandoſi preſſochè ridotta a nulla non ha forza di paſſare dal diſco al piano che combacia ſe non lentiſſimamente.

46. Non andrà però così la biſogna, ſe ripetendo l'eſperienza s'inclini il diſco, e ſi porti a toccare il medeſimo piano in coſta: allora ſuſſiſtendo in quello maggior *tenſione* di elettricità (come ci moſtrerà il fedele elettrometro), giacchè non vien bilanciata che corriſpondentemente ai punti di ſuperficie dell' uno che guardano davvicino la ſuperficie dell' altro, cotal azione elettrica meno indebolita vincerà la piccola reſiſtenza del marmo, o di qualſiaſia altro imperfetto conduttore, e fino di un ſottile ſtrato coibente che troviſi interpoſto, coſicchè l'elettricità trasfonderaſſi realmente, e o s'affiggerà a coteſto ſtrato coibente che copre il conduttore, o

paſſerà entro a queſto ſe è nudo fino a perderſi nel ſuolo *, e ciò in breviſſimo tempo; laddove vedemmo (præc.), che non ne paſſa nulla o quaſi nulla in tempo aſſai più lungo, quando il contatto col medeſimo piano è il più ampio poſſibile. Il che ha l'aria di paradoſſo; ma pur ſi ſpiega così bene coi principj delle atmoſfere elettriche.

47. Quello che ſembra anche più paradoſſo, o almeno che ſorprende dippiù, ſi è che neppure il contatto di un dito, o di un pezzo di metallo comunicanti col ſuolo, replicato più volte e continuato per alcuni ſecondi, valga a ſpogliare intieramente

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* Queſta ſpiegazione bene inteſa ci conduce a render ragione in generale della *virtù della punta*. A parlar giuſto una punta non iſolata, preſentata a un corpo elettrico non ha alcuna virtù propria per attirarne l'elettricità; ella ſi comporta ſemplicemente come un conduttore non iſolato che non oppone reſiſtenza al paſſaggio del fluido elettrico. Se il medeſimo conduttore preſenta al corpo elettrico in vece della punta una palla, od una ſuperficie piana, non oppone già egli per queſto maggiore reſiſtenza; onde è dunque che l'elettricità non vi ſi getta egualmente all'iſteſſa diſtanza dal corpo elettrico? Ciò viene dall'indebolita *tenſione* oſſia azione elettrica di coteſto corpo in virtù della più larga ſuperficie preſentatagli da quel conduttore non iſolato, la quale ſuperficie componendoſi ad un'elettricità contraria, offre maggior *compenſo* che una punta, come ſi è qui ſopra ſpiegato. Adunque in luogo di dimandare perchè una punta tragga o getti ſi da lungi l'elettricità, dovrebbeſi domandare piuttoſto perchè una palla o un piatto egualmente conduttore non lo faccino; allora io farò oſſervare che non è già un difetto di queſta palla o di queſto piano, come non è una virtù propria della punta che metta tale e tanta differenza; ma bene lo ſtato del corpo elettrico e della ſua atmoſfera (con cui intendo anche l'aria che lo circonda *attuata* ad una tenſione di elettricità omologa) il qual decade dalla ſua forte *tenſione* a proporzione che s'immergono in detta ſua atmoſfera e ſi affacciano a lui più punti di un conduttore non iſolato. Affievolita pertanto *l'azione elettrica*, è egli ſorprendente che non poſſa più ſuperare la reſiſtenza di quel lungo ſtrato d'aria interpoſta tra il corpo elettrico ed il conduttore, che ſupera agevolmente quando non preſentandogliſi alla medeſima diſtanza che una punta ſottile, *la tenſione* di eſſo corpo elettrico e dell'aria infinitamente meno bilanciata, ſuſſiſte nel ſuo pieno vigore?

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dell'elettricità il diſco poſato ſull'amico piano; ma ve ne laſci ſovente tanto da poter dare ancora una ſcintilla quando in ſeguito ſi leva eſſo diſco in alto. Invero tal fenomeno ſarebbe ineſplicabile anche nei noſtri principj, ſe il dito o il metallo foſſero perfetti conduttori, a ſegno di non opporre la minima reſiſtenza al paſſagio del fluido elettrico, come ſi crede comunemente; ma la coſa non è coſì; e ce lo dimoſtrano queſte ſteſſe ſperienze. I metalli dunque non ſono che conduttori meno imperfetti degl'altri corpi. Ma, diraſſi, noi vediamo che ſi traſfonde da un capo all'altro di un metallo, e da un metallo all'altro l'elettricità in un iſtante. Sia pure così di quell'elettricità che diſpiega una forza ſenſibile a ſegno di tendere un elettrometro, o di attrarre un filo leggeriſſimo. Ma convien riflettere che al diſotto di queſto vi hanno da eſſere ancora altri gradi di elettricità impercettibili, i quali, dico io, non ſon valevoli a ſuperare ſi toſto quella qualunque piccola reſiſtenza che pure oppor denno i migliori conduttori. Quando dunque un metallo tocca il diſco elettrizzato che ripoſa ſul ſuo piano, lo ſpoglia immantinente dell'elettricità fino al ſegno che la *tenſione* diviene affatto *inſenſibile*, non però *nulla*, eſſendo ridotta ſupponiamo, a $\frac{1}{50}$ di grado. Ma ſe ſollevando il diſco in alto la ſua *capacità* ſi riſtringa a ſegno che diſpieghi una tenſione elettrica 100 e più volte maggiore, queſta ſalirà dunque a 2 gradi ed oltre; con che ſarà divenuta ſenſibile, fin anche al punto di dare una ſcintilla.

48. Fin qui conſiderato abbiamo come l'azione delle atmoſfere elettriche debba modificare l'elettricità del diſco nelle ſue varie ſituazioni, allorchè gli è ſtata infuſa prima di accoſtarlo al piano deferente. Ora vediamo che avvenir debba allorchè gli s'infonde ſtando già egli vicino o meglio applicato al detto piano. Quando ho detto dal bel principio (32.) che in tale ſtato egli ha molto

maggiore *capacità*, e ſon venuto provandolo fin qui, ho detto e provato tutto: le applicazioni ſono facili a farſi. Gioverà non pertanto eſemplificare un eſperienza. Mi ſi dia una boccia di Leyden, o un ampio conduttore elettrizzati a 1 ſol grado di *tenſione*, od anche meno. Se io farò toccare l'una o l'altro al mio diſco poſato, è chiaro che gli comunicheranno della loro elettricità a miſura della ſua *capacità*, tanto cioè quant' egli puo riceverne per comporſi con eſſi ad una *tenſione* oſſia forza elettrica *eguale*, ſupponiamo di ½ grado. Ma la ſua *capacità* or ch' egli è non ſolamente *conjugato* ma combaciante il conduttore compagno, è 100 e più volte maggiore (46.) di quando ſi trova iſolato *ſolitariamente*, oſſia vi vuole per produrvi la data tenſione 100 volte maggior doſe di elettricità (33.), quindi appunto ne avrà preſo 100 volte più, che non avrebbe potuto prenderne ſtando iſolato in aria. Quando dunque ſi leverà in alto a miſura che allontanandoſi dal caro piano ſi ridurrà alla naturale ſua anguſta *capacità*, la *tenſione* elettrica diſpiegheraſſi maggiore, e maggior ſempre fino al termine di 50 gradi (nel ſuppoſto caſo che la tenſione foſſe di ½ grado ſtando il diſco poſato), quando cioè la ſua atmosfera non facendoſi più ſentire al detto piano, ſarà ceſſata ogni maniera di *compenſo*, e tolto quell' *equilibrio accidentale*, che teneva la tenſione così baſſa (39. 42.). E inutile il dire, che calando di nuovo il diſco verſo il piano, ſi abbatterà di nuovo l'elettrometro, a miſura che *l'equilibrio accidentale* ſi andrà riſtabilendo; giacchè queſto è il primo fenomeno che contemplato abbiamo (38.), e che ne ha condotti alla ſpiegazione di tutto il reſto.

49. Soggiugnerò queſto per ultimo ſchiarimento. Succede al diſco che paſſa dallo ſtato d'iſolamento ſolitario a quello di affacciarſi fin anche a combaciare un piano convenientemente preparato, o da queſto all' altro ſtato, lo ſteſſo che ſuccede ad un conduttore

compreſo ſotto anguſta ſuperficie, che ſi diſpieghi in una aſſai più ampia, e *vice verſa* (richiamiamo l'eſempio della catena ammucchiata e poi diſteſa, o dei cilindri ch' entrano un nell' altro (35.). Elettrizzato a un alto grado il conduttore quand' è avvolto e impicciolito, ſe dopo viene a diſtenderſi od allungarſi, decade in lui la *tenſione* a miſura che l'elettricità, compartendoſi a una più grande *capacità*, vien diradata. All' incontro eletrizzato debolmente quando è diſteſo e gode della ſua maggiore capacità, ſe dopo ſi avvolge e rappiccioliſce, va egli acquiſtando viemmagior *tenſione* a miſura che l'elettricità ſi raccoglie e viene condenſata in una capacità minore. Così appunto il noſtro diſco ſe venga elettrizzato quand' è *ſolitario* a una forte *tenſione* queſta anderà ſcemando à miſura ch' egli ſi affacia ad un altro piano non iſolato; all' incontro elettrizzato deboliſſimamente quando è proſſimo a queſto piano o lo combacia, vedraſſi creſcere in lui inſignemente la *tenſione* a miſura che ſi allontana da quel piano. Si puo dunque dire che l'elettricità viene qui pure in certo modo *condenſata*, non altrimenti che nell' addotto eſempio del conduttore che s'impicciolіſce: e quindi il nome di *condenſatore* che ho dato al mio apparecchio. Certo ſe non puo dirſi nel noſtro caſo *condenſata* l'elettricità in minore ſpazio, giacchè e maſſa e volume rimangono i medeſimi nel diſco che adoperiamo, ella è però confinata in tal corpo di cui la *capacità* di grandiſſima che era è divenuta come che ſia picciolіſſima.

50. Ora ſe una debole inſenſibile forza elettrica di una boccetta di Leyden o di un conduttore appena un poco carichi applicata al diſco giacente puo accumularvi tanto di elettricità, onde poi levato in alto diſpieghi una forte tenſione, vibri vivace ſcintilla, &c. che farà una carica forte della boccia o del conduttore applicatavi egualmente? Non farà gran coſa dippiù, per la ragione che tutta quell' elettricità ch' è ſuperiore in forza

alla piccola reſiſtenza che oppone la ſuperficie del piano (46.), ſia perſa, trapaſſando in eſſo (47.). Ad ogni modo ſe queſto piano eſſendo convenientemente preparato (11. 12. 22.), tale reſiſtenza ſia diſcreta, il diſco non ſe ne ſtaccherà ſenza vibrare d'attorno dagl' orli comunque ritondati fiocchi di luce, per la ſtrabocchevole copia di elettricità, di cui ſi troverà carico: e a tanto non ſarà neppur neceſſario che la boccetta che s'impiega a dargliela abbia aſſai forte carica, baſtando una mediocre, e meno che mediocre, tale che appena giunga a dar ſcintilla.

51. Da tutto il fin qui detto s'intende facilmente, che ſe il diſco poſato può prendere buona doſe di elettricità da una boccia di Leyden *, o da un ampio conduttore, comechè deboliſſimamente animati, non lo può in alcun modo da un conduttore poco capace (e come darebbe queſti ciò che non ha?) a meno che non ſi continui d'altra parte ad infondere a lui medeſimo

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* Nella mia memoria ſulla capacità de' conduttori ſemplici dimoſtro la grandiſſima *capacità* che ha una boccia di Leyden comparativamente alla ſua mole, appunto perchè l'elettricità che s'infonde ad una ſuperficie truova un gran compenſo nell' elettricità contraria che prende la ſuperficie oppoſta, ciò che produce la ſolita diminuzione di *tenſione*, &c. Vi fo vedere come 10 pollici quadrati di *ſuperficie armata* hanno una capacità eguale a un conduttore di verghe inargentate lungo preſſo a 100. piedi, il quale ne ha una grandiſſima, talchè le ſue ſcintille producono la vera *commozione* in un grado abbaſtanza forte. Ivi anche accenno come tutti i fenomeni della carica e della ſcarica degli ſtrati iſolanti, dell' elettroforo, delle punte ec. poſſono dipendere dall' iſteſſa azione delle atmoſfere elettriche, combinata, per ciò che appartiene agli ſtrati iſolanti, con una certa non molto grande reſiſtenza che prova l'elettricità ad affiggerſi alla ſuperficie di queſti egualmente che a ſortirne, e con quella incomparabilmente più grande e può dirſi inſuperabile che la impediſce di diffonderſi attraverſandone la ſpeſſezza. Intorno a che fin dal tempo in cui pubblicai la deſcrizione, e le principali ſperienze del mio elettroforo, che fu nel 1775 (vegg. la Scelta d' Opuſc. intereſſ. di quell' anno) io avea promeſſo di eſporre tutte le mie idee in un trattato che avrebbe per titolo: *dell' azione delle atmoſfere elettriche, e de' fenomeni che ne derivano negli ſtrati iſolanti.*

quella qualunque debole elettricità, a meno che la ſorgente non continui per qualche tempo: il che ha luogo per eſempio nel conduttore atmosferico che bee l'elettricità inſenſibile dell' aria, e in quello maliſſimo iſolato d'una macchina ordinaria, il di cui giuoco vi mantiene una ſi debole *tenſione* di elettricità, che in niun modo appara. In ambi queſti caſi abbiamo oſſervato infatti (4. 25.) che vi vuol del tempo prima che il diſco poſſa raccorre una doſe ſufficiente di elettricità.

52. Come un ampio conduttore traſmette la maſſima parte della ſua elettricità al noſtro diſco, il quale quantunque aſſai più picciolo, gode però in grazia della ſua vantaggioſa poſizione, in grazia di quell' *equilibrio accidentale* a cui ſi compone col piano, d'una *capacità* molto più grande di quella che gli compete in iſtato ſolitario; e come levando in ſeguito eſſo diſco in alto, con che tolto ogni *equilibrio* o *compenſo*, vien riſtretto alla naturale ſua anguſta capacità, quella ſteſſa doſe di elettricità preſa al gran conduttore, e che appunto per eſſer egli ſi grande vi producea ſi debole *tenſione*, or ne produce una tanta più grande in coteſto diſco; nell' iſteſſa maniera, e per l'egual ragione l'elettricità aumenterà una ſeconda volta di tenſione facendola paſſare dal diſco già ſollevato ad un altro giacente molto più piccolo, da innalzarſi quindi ſimilmente.

Il Sig. CAVALLO, a cui dietro le altre mie ſperienze, ſuggerì queſt' artificio, ha fatto tal picciolo diſco d'una laminetta non più grande d'uno ſcillino. E certo queſto ſecondo *condenſatore* dell' elettricità e utile in molti caſi in cui l'elettricità non è ſenſibile ancora o dubbia col primo: come ce ne hanno aſſicurato varie prove che facemmo inſieme. Talora l'ordinario diſco toccato dal corpo, di cui ſi dubitava ſe aveſſe o no un principio di elettricità, non movea ancora l'elettrometro ſenſibiliſſimo dell' iſteſſo Sig. CAVALLO; ma toccato con quel diſco l'altro picciolino, queſto facea divergere ſenſibilmente le palottoline dell' elettrometro. Eppure qualche volta anche con queſto non ſi otteneva nulla, o un' ombra ſolamente di elettricità. Or ſe noi ſupponiamo la *tenſione* elettrica accreſciuta a 1000 volte tanto per l'intervento dei due condenſatori, il che non è troppo, quanto mai debole eſſer dovea originariamente nel corpo eſaminato? Quanto debole p. e. quella che ſi eccita in un metallo ſtrofinandolo colla mano nuda, giacchè communicata al primo, e da queſto al ſecondo picciolo diſco, e finalmente all' elettrometro, le palle appena fan cenno di ſcoſtarſi? Ma baſta che facciano tanto per eſſer noi convinti, che l'elettricità non è nulla, e che il metallo l'ha originariamente contratta per lo ſtroppicciamento della mano. Quanto mai eravam lontani da una ſimile ſcoperta pochi anni addietro prima del noſtro *condenſatore*, e dell' elettrometro così ſenſibile del Sig CAVALLO. Quanti gradi di elettricità noi ſcopriamo adeſſo al diſotto del più picciolo d'allora?

APPENDICE.

HO detto al § 28. che mi è riuſcito finalmente di ottenere ſegni diſtintiſſimi di elettricità e dalla ſemplice evaporazione dell' acqua, e da varie effervescenze chimiche. Eſſendo queſto un fatto non meno intereſſante che nuovo, ſtimo non inopportuno di far qui il racconto fedele delle ſperienze. Le prime dunque, come ivi accenno, ſono ſtate fatte a Parigi in compagnia di due fiſici illuminati e membri dell' Acc. R. delle Scienze. Furono queſti il Sig. LAVOISIER, e il Sig. DE LA PLACE. Eglino concepiron meco la ſperenza di un felice riuſcimento quando ebbi loro moſtrato gli effetti del mio *condenſatore*, e ſpiegata la ragione dei fenomeni: conſequentemente il Sig. LAVOISIER ne ordinò un grande col piano di marmo bianco. I primi tentativi da me fatti con queſto in compagnia del Sig. DE LA PLACE ſull' evaporazione dell' acqua e dell etere non furono coronati dal ſucceſſo; ma il tempo era cattivo, la ſtanza troppo picciola e ingombrata di vapori, e l'apparato non troppo ben in ordine. All incontro quelli che ripeterono l'iſteſſo Sig. DE LA PLACE e Sig. LAVOISIER ad una campagna di queſt' ultimo ebbero buon riuſcimento. La qual coſa c'invogliò a ripetere e moltiplicar le ſperienze, e il ſucceſſo fu completo, avendo ottenuto ſegni chiariſſimi di elettricità dall' evaporazione dell' acqua, dalla ſemplice combuſtione dei carboni, e dall' effervescenza delle limature di ferro nell' acido vitriolico diluto. Ciò avvenne il giorno 13 Aprile e la maniera di far l'eſperienza fu queſta: ſi iſolò in un aperto giardino una gran laſtra di metallo, alla quale era attaccato un lungo filo di ferro che veniva a terminare in contatto dello ſcudo o diſco poſato ſul piano di marmo, e queſto tenevaſi continuamente aſciutto e caldo da alquanti carboni ſottopoſti. Ciò fatto poſimo ſu la detta laſtra iſolata alcuni ſcaldini ripieni di carboni mezzo acceſi, e laſciammo che la combuſtione ajutata da un gentil vento che ſpirava andaſſe rinforzandoſi per alcuni minuti: allora rimovendo lo ſcudo dal contatto del filo metallico e quindi da quello dal marmo con alzarlo al conſueto modo, vi comparvero i ſegni aſpettati di elettricità, mentre accoſtato al nuovo elettrometro del Sig. CAVALLO, fece che s'apriſſero i due fili colle pallottoline: eſaminata queſta elettricità ſi trovò eſſere *negativa*. Si ripetè l'eſperienza ponendo ſulla laſtra iſolata invece dei ſcaldini quattro vaſi con entro limatura di ferro e acqua, quindi verſando in tutti quattro a un tempo abbaſtanza d'olio di vitriolo per far ſorgere una furioſa efferveſcenza: quando il più forte bollore cominciava a cadere, allora fu che eſplorato lo ſcudo non che movere i fili dell' elettrometro a qualche diſtanza, ci diede una ſenſibile ſcintilla. Anche qui l'elettricità ſi riconobbe eſſere *negativa*. Quanto furon vivi e diſtinti i ſegni elettrici con tal prova dell' efferveſcenza, altrettanto deboli ed equivoci riuſcirono queſta volta coll' evaporazione dell' acqua eccitata or con mettere delle caſſerole con entro acqua a bollire ſopra i ſcaldini portati come qui innanzi dalla laſtra iſolata, ora con verſar l'acqua in coteſte caſſerole previamente ben riſcaldate.

Pochi giorni dopo ripetemmo le ſperienze in una grande ſtanza eſtendendole alle altre efferveſcenze che producono l'aria fiſſa, e l'aria nitroſa, con buon ſucceſſo: l'evaporazione ſola dell' acqua produſſe ſegni deboliſſimi talchè ebbimo pena a determinare di quale ſpecie foſſe l'elettricità; anzi di tre volte, due ci parve che foſſe *poſitiva*; ma v' è luogo a credere, ed io giudico certamente, che ſia ſtato un errore.

Ancor paſſati alcuni giorni ſi ritornò alle ſperienze eſſendo di compagnia anche il Sig. LE ROY membro eſſo pure dell' Accademia R.; ma nè la combuſtione, nè l'evaporazione dell' acqua non ci dieder ſegni ſenſibili: di che accagionammo l'eſſer l'aria umidiſſima per il tempo piovoſo che faceva. Pur ne ottenemmo colla generazione dell' aria infiammabile nel momento della più viva efferveſcenza: e ſe l'elettricità non fu queſta volta così forte da ſcintillare, lo fu abbaſtanza perchè ne diſtingueſſimo chiariſſimamente la ſpecie, che era *negativa*.

Prima di laſciar Parigi (che fu il 23 Aprile) volendo io moſtrare qualche ſperienza di queſto genere ad un amatore di elettricità e valente machiniſta, il Sig. BILEAUM, una volta che mi trovai nel ſuo laboratorio, preſi una giara di vetro, e ſoſpeſala a un cordoncino di ſeta vi miſi i materiali per la produzione dell' aria infiammabile: avea fatto entrare nella giara medeſima un filo di ferro in modo che toccaſſe la limatura e l' altro ſuo capo ſporgente veniſſe a communicare coll' elettrometro ſenſibiliſſimo del Sig. CAVALLO. Quando l'efferveſcenza fu ſalita al ſommo e la ſpuma ſormontava i labbri del vaſo, le palle, ſcoſtandoſi, dieder ſegno di elettricità; nè queſta fu così debole, che non poteſſe conoſcerſi eſſer *negativa*.

Le ſperienze coll' evaporazione dell' acqua, che non avean troppo bene corriſpoſto a Parigi, ebbero molto miglior ſucceſſo a Londra, quando mi ſuggerì l'eſpediente di gettere dell' acqua ſopra i carboni acceſi ch' erano in uno ſcaldino iſolato. L'effumazione rapida che ſuccede non manca mai di elettrizzare lo ſcaldino *negativamente*, il quale dà ſegni abbaſtanza ſenſibili col ſolo elettrometro, e col condenſatore, ſe è ben preparato, arriva a produr ſcintille. Si trovarono preſenti la prima volta a queſte ſperienze in caſa del Sig. BENNET grand' amatore di elettricità, il Sig. CAVALLO e il Sig. KIRWAN membri della S. R. e il Sig. WALKER lettore di fiſica. Ci ſervimmo per apparecchio condenſatore d'un picciolo ſcudo d'elettroforo, e d'un piattello di legno, che ſi trovò al giuſto punto ſemicoibente, il che è raro quando il legno non è inverniciato.

Un' altra volta in caſa del Sig. CAVALLO riuſcì l'eſperienza iſolando un picciolo crogiuolo con entro due o tre carboni acceſi e quindi verſandovi un cucchiajo d'acqua: un filo di ferro che toccava i carboni, ed eſtendevaſi fino all' elettrometro, vi portò ſenſibile elettricità e ſempre *negativa*.

Queſte ſono le ſperienze, che fino ad ora ho avuto occaſione di fare; intorno alle quali non debbo tralaſciar di dire, che ſebbene non aveſſimo ſempre biſogno dell' apparecchio *condenſatore* (il quale, ſe non è beniſſimo in ordine, a nulla ſerve, e puo nuocere anzichè giovere) per aver ſegni non dubbi, il ſolo elettrometro ſenſibiliſſimo del Sig. CAVALLO avendoci baſtato più volte; convien però confeſſare che ſi fu quell' apparecchio che ci miſe ſulla via di tali ſperienze, e che col mezzo ſuo ſolamente potemmo ottenere ſegni di una certa forza, e fin la ſcintilla elettrica. Io non dubito che eſſendo ora reſe così facili tali ſperienze, non ſiano per eſſere e ripetute e promoſſe. Il campo e ſolamente aperto, e molto reſta ancora a fare. Se i corpi riſolvendoſi in vapori o in un fluido elaſtico ſi caricano di fuoco elettrico a ſpeſe degl' altri corpi, e gli elettrizzano per conſeguenza *negativamente*, venendo in ſeguito i vapori medeſimi a condenſarſi, non cercheranno eſſi di deporre queſto carico, e non produrranno conſeguentemente ſegni di elettricità *poſitiva*? Ecco ciò che merita ſingolarmente d'eſſere verificato coll' eſperienza. Io ho già immaginato diverſi modi di tentare la coſa, che metterò alla prova toſto che ne abbia il commodo. Intanto mi ſia qui permeſſo di dar corſo per un momento alle idee che volgo in mente intorno all' elettricità atmosferica.

Le ſperienze fatte fin quì, e che abbiamo riferite, benchè non ſian molte, tutte però concorrono a moſtrarci che i vapori dell' acqua, e generalmente le parti d'ogni corpo, che ſi ſtaccano volatilizzandoſi, portano via ſeco una quantità di fluido elettrico a ſpeſe dei corpi fiſſi che rimangono, elettrizzandoli con ciò *negativamente*, non altrimenti che ne portan via una quantità di fuoco elementare, con ciò raffreddandoli. Quindi volli inferire che i corpi riſolvendoſi in vapori, o prendendo l'abito aereo, acquiſtino una maggiore capacità riſpetto al fluido elettrico, giuſto come l'acquiſtano maggiore riſpetto al fuoco comune o fluido calorifico. Chi non ſarà colpito da così bella analogia, per cui l'elettricità porta del lume alla novella dottrina del calore, e ne riceve a vicenda? Parlo della dottrina dal calor *latente* o *ſpecifico*, come ſi vuol chiamare, di cui BLACK e

WILKE colle ſtupende loro ſcoperte han gettato i ſemi, e che è ſtata ultimamente tanto promoſſa dal Dr. CRAWFORD dietro le ſperienze del Dr. IRWINE.

Seguendo queſta analogia ſiccome i vapori allorchè ſi condenſano e ritornano in acqua, e conſeguentemente alla primiera più anguſta capacità, perdono il lor calore *latente*, oſſia depongono il dippiù di fuoco che ſi avevano appropriato volatizzandoſi; così pure daran fuori il fluido elettrico divenuto ora ridondante. Ed ecco come naſce l'elettricità di ecceſſo, che domina ſempre più o meno nell' aria anche ſerena, a quell' altezza che i vapori cominciano a condenſarſi; la quale è più ſenſibile nelle nebbie, ove quelli ſi condenſano maggiormente, e già ſi figurano in goccie; e infino fortiſſima laddove le folte nebbie ſi agglomerano in nubi. Fin quì l'elettricità dell' atmosfera ſarà ſempre *poſitiva*. Ma formata che ſia una nube potentemente elettrica in più, ella avrà una sfera di attività intorno ad eſſa, nella quale ſe avviene ch' entri un' altra nube, allora giuſta le note leggi delle atmosfere, gran parte del fluido elettrico di queſta ſeconda nube ſi ritirerà verſo l'eſtremità più lontana dalla prima, e potrà anche ſortirne ove incontri o altra nube, o vapori, o prominenze terreſtri che lo poſſan ricevere: ed ecco una nube elettrizzata *negativamente*, la quale potrà a ſua poſta occaſionare coll' influſſo della propria atmosfera l'elettricità *poſitiva* in una terza, &c. di queſta maniera s'intende beniſſimo come ſi poſſano avere ſovente nè conduttori atmosferici ſegni di elettricità *negativa* a celo più che coperto; e come ne' temporali ſpecialmente, ove molte nubi ſi veggono penſili e ſtaccate vergere al baſſo, e or ondeggiare per qualche tempo, ora ſcorrere le une ſotto le altre, or traſportarſi rapidamente, l'elettricità cambj più volte, e ſpeſſo a un tratto da *poſitiva* in *negativa*, e vice-verſa.

Or anche non fia più ſtupore che le eruzioni de vulcani, ſiano ſtate ſovente accompagnate da fulmini in iſpecie. Quella ſtrepitoſiſſima del Veſuvio dell' anno 1779, in cui infinite ſaette ſi ſon vedute guizzare entro gl' immenſi globi di fumo eruttati. Le poche ſperienze fatte mi han dato a vedere che la quantità di elettricità prodotta dalle effumazioni, dipenda molto e dalla copia dei fumi che s'alzano e ſingolarmente dalla rapidità. Or quale e quanta non dee eſſere l'elettricità in ſimili eruzioni?

XVII. *Extract of a Regiſter of the Barometer, Thermometer, and Rain, at* Lyndon, *in* Rutland, 1780. *By* Thomas Barker, *Eſquire.*

Read April 11, 1782.

		Barometer.			Thermometer.						Rain.
					In the Houſe.			Abroad.			
		Higheſt.	Loweſt.	Mean.	High.	Low.	Mean	High.	Low.	Mean	
Jan.	Morn.	30,15	28,55	29,45	46½	32	38	47	21	32	2,264
	Aftern.				46	33	38	50½	25	36	
Feb.	Morn.	29,82	28,32	29,29	46½	38	42½	45	30	37½	1,664
	Aftern.				47	39	43	52½	36½	44	
Mar.	Morn.	30,01	29,46	29,74	51	40	46	44½	30	38	0,160
	Aftern.				53½	42	47½	56½	40	48	
Apr.	Morn.	29,88	28,92	29,43	57	41	50	53	33½	43	1,938
	Aftern.				58	43	51½	64	39	54	
May	Morn.	29,95	29,20	29,60	66	47	54	64	38	47	0,974
	Aftern.				70	48	55	76½	47½	59½	
June	Morn.	29,88	29,00	29,40	69	58	62	69	52	60	2,958
	Aftern.				73	59	63½	79½	57	67	
July	Morn.	29,96	29,22	29,59	68	61	64	67	49	58½	1,683
	Aftern.				71½	62	66	79½	62	70½	
Aug	Morn.	29,89	29,02	29,45	70½	60	65	64	49½	57	1,097
	Aftern.				72½	61	66½	80	61	71	
Sept.	Morn.	29,86	29,04	29,45	65	50½	59	63	38	52	4,004
	Aftern.				67	51	60	80	50½	63	
Oct.	Morn.	30,00	28,96	29,67	59	44	52½	59	26½	44	0,081
	Aftern.				61	44	53	66	41	53½	
Nov.	Morn.	29,74	28,49	29,22	51	40½	45	52	28	39	2,296
	Aftern.				51½	41	46	54	35	45	
Dec.	Morn.	29,68	29,00	29,34	50	37½	43½	51	28½	39	1,703
	Aftern.				51	37	44	53	33½	42½	
											20,822

The ground was unuſually dry the firſt half of January, and want of water in ſome places. Inclined to froſt, but not fixed; one for a week was followed toward the end of the month by a ſmarter but ſhorter with a good deal of ſnow, and went away with rain and floods; then ſhowery, and in February often ſtormy. The month of March was drier, generally fine, a good ſeed-time, and the middle of the month warm and growing; but cold N.E. winds at the end. Soon after April came in, the weather was fine and growing, ſometimes ſhowery, and the middle being very warm brought on things very much; but the end of the month, and beginning of May, cold N.E. winds, yet ſometimes hot ſun. About the middle of May was ſhowery and growing; but the end of it, and beginning of June, hot, dry, clear, and burning. June 3d, began ſhowers and often thunder, which being ſeveral times repeated for a fortnight, and hot weather with it, made plenty of graſs in this country and ſome others; but where the rains were ſmaller, as they were about London, in Suſſex, and other places, it only freſhened the ground without growing much. The beginning of hay-time was very fine, a wet week in the middle did not greatly hurt the hay, and was very good for the turnips, then newly ſown. The latter half of July, and almoſt all Auguſt, were very dry, hot, and burning; the harveſt almoſt all very well got, except ſome of the peaſe, which were either not got in, or the hovels not thatched, when the great rain came September 2. There was a great crop of wheat, but in ſome places it was mildewed; the barley was univerſally good; the peaſe uncertain. The general prices were, wheat thirty-eight ſhillings, barley ſixteen or ſeventeen ſhillings, oats eleven or twelve ſhillings, peaſe and beans about twenty-two or twenty-three ſhillings a quarter.

The ground was now exceedingly burnt, and great want of water; but in the firſt half of September there came ſo much rain, the ground already warm, and hot weather with it, that in a fortnight's time there was plenty of graſs. The autumn was in general fine and pleaſant, and the latter end of September and all October ſcarce any rain, ſo that at the end of the month the graſs in ſome places plainly began to burn, which was remarkable ſo late in the year, and after its being ſo fruitful in September; and notwithſtanding ſome ſhowery weather in the former part of November, the roads were rather duſty the beginning of December; then dark, miſty, raw, cold eaſt winds, but milder again and ſhowery till the end of the year. It has been an uncommonly open ſeaſon, there have been ſome froſty mornings, and one day or two it ſcarce went away in the ſhade; but there has not been one thorough froſty day yet, and there has been thunder at Chriſtmas.

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THE ROYAL SOCIETY

BY ORDER OF THE

PRESIDENT AND COUNCIL.

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for January 1781.

	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Jan. 1	8	0	45,0	47,0	29,80		SW	1	Cloudy.
	2	0	46,0	47,0	29,71		SW	1	Cloudy and rain.
2	8	0	34,5	43,5	29,57	0,045	W by N	1	Fair.
	2	0	40,0	45,0	29,61		NW	1	Fine.
3	8	0	33,5	38,0	29,76		N by W	1	Fair.
	2	0	38,0	39,0	29,88		N by W	1	Fine.
4	8	0	28,0	35,0	30,01		NW	1	Fair and froſty.
	2	0	34,0	36,0	30,05		NW	1	Cloudy.
5	8	0	25,0	32,0	30,20		SSW	1	Froſty.
	2	0	32,0	33,0	30,23		SW	1	Froſty.
6	8	0	35,0	33,5	30,21		SSW	1	Cloudy.
	2	0	40,0	36,5	30,25		SW	1	Cloudy.
7	8	0	38,5	37,0	30,22		SSW	1	Cloudy.
	2	0	46,0	40,5	30,10		W by S	1	Fair.
8	8	0	40,5	41,5	30,31	0,042	N by E	1	Fair.
	2	0	42,5	41,5	30,39		ENE	2	Cloudy.
9	8	0	32,5	37,5	30,54		NNE	2	Fair.
	2	0	37,5	38,5	30,55		NE	2	Fine.
10	8	0	32,0	34,0	30,52		E by N	2	Cloudy.
	2	0	33,5	35,0	30,50		E by N	2	Cloudy.
11	8	0	32,0	33,5	30,38		E by N	3	Cloudy.
	2	0	31,0	33,5	30,32		NE	3	Cloudy.
12	8	0	31,5	32,0	30,26		ESE	1	Foggy.
	2	0	35,0	35,0	30,24		SE	1	Cloudy.
13	8	0	33,0	34,0	30,11		ENE	1	Foggy.
	2	0	35,0	35,0	30,16		NE	1	Cloudy.
14	8	0	29,0	32,0	30,04		E by N	1	Foggy.
	2	0	34,0	35,0	29,98		ENE	1	Cloudy.
15	8	0	30,5	33,5	29,92		ENE	1	Foggy.
	2	0	36,0	35,0	29,89		NE	1	Fine.
16	8	0	31,0	33,5	29,89		ENE	1	Foggy.
	2	0	33,5	33,5	29,83		NE	1	Cloudy.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Jan. 17	8	0	32,5	33,0	29,82		ENE	1	Foggy.
	2	0	38,0	35,0	29,76		NE	1	Rainy.
18	8	0	40,0	37,0	29,57	0,190	E by N	1	Rainy.
	2	0	45,0	39,0	29,56		NE	1	Cloudy.
19	8	0	37,5	40,0	29,78	0,221	NNE	1	Fine.
	2	0	41,0	41,5	29,86		NNE	1	Fine.
20	8	0	34,0	38,0	30,05		SE	1	Foggy.
	2	0	41,0	40,0	29,99		SE	1	Fine.
21	8	0	44,0	40,0	29,45		SW	1	Foggy.
	2	0	41,5	42,5	29,44		NNE	1	Rainy.
22	8	0	32,0	38,0	29,65	0,073	SE	1	Fair.
	2	0	33,0	38,0	29,66		SE	1	Fine.
23	8	0	26,0	32,0	29,39		ENE	1	Foggy.
	2	0	32,0	34,0	29,37		SE	1	Cloudy with ſnow.
24	8	0	35,5	34,0	29,16		SW	1	Rainy.
	2	0	41,0	37,0	29,04		SW	1	Rainy.
25	8	0	31,5	33,0	29,32		NW	1	Fair.
	2	0	35,0	36,0	29,52		NW	1	Fine.
26	8	0	35,0	34,0	29,09	0,485	SSE	1	Rainy.
	2	0	37,0	38,0	29,23		SE	1	Cloudy.
27	8	0	26,5	32,5	30,05		WSW	1	Fine.
	2	0	36,0	37,0	30,13		SW	1	Fine.
28	8	0	46,5	37,0	29,93	0,063	SW	2	Cloudy.
	2	0	51,5	42,0	29,94		SSW	2	Cloudy.
29	8	0	46,5	43,0	29,89		SSW	2	Cloudy.
	2	0	50,5	46,0	29,89		SW	2	Cloudy.
30	8	0	47,0	47,5	29,75	0,229	SW	2	Cloudy.
	2	0	48,0	49,0	29,81		SW	3	Showery.
31	8	0	35.5	43,5	30,22		WNW	1	Fine.
	2	0	45 0	44,0	30,22		SW	1	Fine.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Feb. 1	8	0	42,0	43 0	30,05	0,110	SSW	1	Rainy.
	2	0	49,0	46,0	30,01		NW	1	Fine.
2	8	0	40,5	43,0	30 19	0,120	SW	1	Foggy.
	2	0	46,0	46,0	30,12		NW	1	Rainy.
3	8	0	41,5	44,0	30,33		SSW	1	Cloudy.
	2	0	43,0	46,0	30,34		SW	1	Cloudy.
4	8	0	43,0	46,0	30,34		SW	1	Cloudy.
	2	0	48,0	47,0	30,05		SW	1	Fine.
5	8	0	44,5	44,0	29,78		SW	1	Fine.
	2	0	50,0	49,0	29,75		SW	1	Cloudy.
6	8	0	46,5	49,0	29.88		SW	1	Cloudy.
	2	0	52,0	51,0	29,96		SW	1	Fine.
7	8	0	48,5	46,5	30,03		WSW	1	Fine.
	2	0	40,0	43,0	29,73		SW	2	Cloudy.
8	8	0	47,5	48,5	29,51	0,233	SSE	2	Rainy.
	2	0	52,0	50,5	29,59		SW	2	Fine.
9	8	0	40,0	46,0	29,82		SW	1	Fine.
	2	0	52,5	50,0	29.83		SW	1	Cloudy.
10	8	0	48,0	49,0	29,88		SSW	3	Cloudy.
	2	0	50,0	50,0	29,74		SW	3	Cloudy.
11	8	0	42,5	47,0	29,58	0,081	SW	2	Fine.
	2	0	51,0	50,5	29 59		SW	2	Fine.
12	8	0	47,0	49,0	28,95	0,430	SW	3	Cloudy.
	2	0	44,0	46,0	29.96		SW	1	Cloudy.
13	8	0	43,0	45 0	29,36		SW	2	Cloudy.
	2	0	47,0	47,5	29,39		NW	2	Fine.
14	8	0	39,5	45.0	29 59	0,150	WSW	1	Cloudy.
	2	0	44,5	44,0	29.52		NW	2	Fine.
15	8	0	35,0	41,0	29,98		NW	1	Fine.
	2	0	43 5	44,0	30,05		SW	1	Fine.
16	8	0	33,0	37,0	30,09		WSW	1	Fine.
	2	0	44,0	42,0	30,02		NW	1	Fine.

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	Time.	Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H. M.			Inches.	Inch.	Points.	Str.	
Feb. 17	8 0	31,5	37,5	30,12		NW	1	Fine.
	2 0	42,0	44,0	30,07		NW	2	Fine.
18	8 0	31,0	36,0	30,09		N by W	1	Fair.
	2 0	43,5	40,5	30,03		SW	1	Cloudy.
19	8 0	37,0	39,0	29,79		NW	1	Fine.
	2 0	43,5	42,5	29,81		NW	1	Cloudy.
20	8 0	37,0	39,0	30,22		SW	1	Cloudy.
	2 0	42,5	41,5	30,23		NE	1	Fine.
21	8 0	34,0	38,0	30,16		NNE	1	Cloudy.
	2 0	35,5	39,5	30,09		NE	1	Rainy.
22	8 0	33,0	36,0	30,12	0,073	NNE	1	Fine.
	2 0	41,5	39,0	30,14		NNE	1	Fine.
23	8 0	37,5	37,0	30,03		NW	1	Cloudy.
	2 0	43,5	41,0	29,89		NW	1	Cloudy.
24	8 0	40,5	42,0	29,71	0,131	SW	1	Cloudy.
	2 0	49,0	44,0	29,71		SW	1	Rainy.
25	8 0	43,0	45,0	29,17	0,150	NW	1	Fine.
	2 0	44,0	46,5	29,19		NW	1	Fine.
26	8 0	37,0	41,0	29,08		SW	1	Cloudy.
	2 0	41,5	43,0	29,08		SW	1	Fine.
27	8 0	39,0	41,0	29,26	0,072	SW	1	Cloudy.
	2 0	45,0	47,0	29,88		NW	3	Cloudy.
28	8 0	37,5	41,0	29,79	0,126	NW	1	Fine.
	2 0	45,0	46,0	29,97		NW	1	Fine.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Mar. 1	7	0	35,0	41,5	30,19		SW	1	Fine.
	2	0	47,0	43,5	30,06		SW	1	Rainy.
2	7	0	41,0	43,0	30,26	0,120	WSW	1	Cloudy.
	2	0	50,5	47,0	30,26		NW	1	Fine.
3	7	0	48,0	49,0	30,26		SW	1	Cloudy.
	2	0	55,0	51,0	30,26		WSW	1	Cloudy.
4	7	0	47,0	51,0	30,18		SW	1	Cloudy.
	2	0	50,5	51,5	30,10		SW	1	Cloudy.
5	7	0	45,5	51,5	30,14		SW	1	Cloudy.
	2	0	51,0	51,5	30,22		NE	1	Cloudy.
6	7	0	37,0	47,0	30,36	0,002	SW	1	Fine.
	2	0	52,5	51,0	30,33		SW	1	Fine.
7	7	0	35,5	45,5	30,25		SW	1	Cloudy.
	2	0	51,0	49,0	30,19		WSW	1	Fine.
8	7	0	37,5	40,0	30,11		SSW	1	Rainy.
	2	0	48,0	47,5	30,19		SSW	1	Cloudy.
9	7	0	43,0	48,5	30,22	0,170	SW	1	Fine.
	2	0	55,0	50,5	30,17		SSW	1	Fine.
10	7	0	44,5	49,5	30,17		WSW	1	Fine.
	2	0	55,0	52,0	30,17		NW	1	Cloudy.
11	7	0	38,5	48,5	30,19		NNE	1	Fine.
	2	0	51,5	50,0	30,21		NNE	1	Fine.
12	7	0	32,0	42,0	30,27		SE	1	Cloudy.
	2	0	42,0	46,0	30,28		NW	1	Cloudy.
13	7	0	41,0	44,5	30,32		NE	1	Fine.
	2	0	49,5	48,0	30,31		SE	1	Fine.
14	7	0	32,5	38,5	30,33		NE	1	Cloudy.
	2	0	45,5	44,0	30,33		ESE	1	Fine.
15	7	0	33,5	37,0	30,44		NE	1	Fine.
	2	0	48,5	47,0	30,46		ENE	1	Fine.
16	7	0	36,0	37,5	30,39		ENE	1	Cloudy.
	2	0	46,5	46,0	30,34		ENE	1	Fine.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Mar. 17	7	0	39,0	38,5	30,22		ESE	1	Cloudy.
	2	0	50,5	48,0	30,16		ENE	1	Fine.
18	7	0	33,0	41,5	30,09		SSE	1	Foggy.
	2	0	57,0	47,0	30,06		WSW	1	Fine.
19	7	0	39,0	46,5	30,14		NW	1	Fine.
	2	0	55,0	49,0	30,14		WSW	1	Fine.
20	7	0	41,5	44,5	30,22		NNW	1	Foggy.
	2	0	60,6	51,0	30,22		NW	1	Fine.
21	7	0	45,0	50,0	30,18		W by S	1	Fine.
	2	0	57,5	55,0	30,16		WSW	1	Fine.
22	7	0	41,5	46,5	30,19		WSW	1	Fine.
	2	0	58,0	53,5	30,18		SW	1	Fine.
23	7	0	41,5	46,5	30,38		SE	1	Fine.
	2	0	51,0	50,5	30,35		ENE	1	Fine.
24	7	0	36,0	42,0	30,47		NW	1	Foggy.
	2	0	56,0	49,0	30,47		SE	1	Fine.
25	7	0	38,0	46,0	30,46		NE	1	Fine.
	2	0	60,5	51,0	30,29		SW	1	Fine.
26	7	0	39,0	43,0	29,91		WSW	1	Fine.
	2	0	47,0	50,5	30,01		NE	2	Rainy.
27	7	0	48,5	43,5	30,02		NW	1	Fine.
	2	0	49,0	44,0	29,93		NW	2	Fine.
28	7	0	34,0	39,0	29,88		NE	1	Fine.
	2	0	48,0	45,0	29,89		NE	2	Fine.
29	7	0	34,0	37,0	29,92		NE	2	Cloudy.
	2	0	43,0	42,0	29,91		NE	2	Fine.
30	7	0	35,5	37,5	29,91		NE	2	Fine.
	2	0	47,0	42,0	29,91		NE	2	Fine.
31	7	0	35,5	35,0	30,09		ENE	1	Foggy.
	2	0	34,0	43,5	30,09		NE	2	Cloudy.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Apr. 1	7	0	37,5	38,0	29,91		ESE	1	Fine.
	2	0	37,0	43,0	29,81		E by S	1	Fine.
2	7	0	37,0	42,0	29,74		SW	1	Fine.
	2	0	53,5	47,0	29,83		ENE	1	Fine.
3	7	0	37,5	44,5	29,98		NE	1	Fine.
	2	0	53,5	52,0	29,94		SE	1	Fine.
4	7	0	38,0	43,0	29,76		NE	1	Cloudy.
	2	0	43,5	44,0	29,63		NE	1	Cloudy.
5	7	0	40,0	43,5	29,34		SE	1	Cloudy.
	2	0	51,0	48,0	29,29		SE	1	Cloudy.
6	7	0	45,0	48,5	29,37		SW	1	Cloudy.
	2	0	50,5	49,5	29,38		SW	1	Fine.
7	7	0	46,0	49,0	29,57		SW	1	Fine.
	2	0	51,0	51,5	29,51		SE	1	Rainy.
8	7	0	48,0	50,5	29,53	0,150	SW	2	Cloudy.
	2	0	50,0	65,0	29,68		SW	2	Fine.
9	7	0	53,0	54,0	29,72	0,200	SW	2	Rainy.
	2	0	50,0	60,0	29,82		NW	2	Fine.
10	7	0	54,0	57,5	29,74		SW	1	Rainy.
	2	0	62,5	60,0	29,76		SW	2	Fine.
11	7	0	49,0	51,5	29,73	0,140	SE	1	Fine.
	2	0	57,0	56,0	29,66		SE	1	Cloudy.
12	7	0	49,0	54,5	29,52		NW	1	Cloudy.
	2	0	61,5	58,0	29,64		NW	2	Cloudy.
13	7	0	39,5	51,0	29,96	0,095	SSW	1	Fine.
	2	0	54,0	53,5	29,99		SW	1	Fine.
14	7	0	40,0	45,0	29,91		WSW	1	Fine.
	2	0	58,0	52,5	29,79		SW	1	Fine.
15	7	0	46,0	47,0	29,79		SW	1	Fine.
	2	0	62,0	66,5	29,81		SW	1	Fine.
16	7	0	46,0	49,0	29,88		SW	1	Fine.
	2	0	53,0	58,5	29,86		SSW	1	Fine.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Apr. 17	7	0	49,0	53,5	29,91		SW	1	Fine.
	2	0	66,5	61,0	29,92		SSE	1	Fine.
18	7	0	48,0	53,0	29,94		ENE	1	Fine.
	2	0	65,0	62,5	29,91		ESE	1	Fine.
19	7	0	55,5	57,5	29,93		SSW	1	Fine.
	2	0	65,0	62,5	29,99		WSW	1	Fine.
20	7	0	51,0	67,0	30,02	0,065	SSE	1	Cloudy.
	2	0	64,0	64,0	30,09		W	1	Fine.
21	7	0	55,0	58,0	30,18		SSW	1	Rainy.
	2	0	64,0	63,0	30,16		SW	1	Cloudy.
22	7	0	55,5	58,0	30,09		SSW	1	Fine.
	2	0	66,0	64,0	30,05		SW	1	Fine.
23	7	0	48,0	52,0	30,07		SW	1	Fine.
	2	0	57,0	57,0	30,01		NW	1	Cloudy.
24	7	0	45,0	47,0	30,16		NNE	1	Cloudy.
	2	0	51,0	55,0	30,22		NE	2	Cloudy.
25	7	0	44,5	50,0	30,24		NW	1	Fine.
	2	0	46,5	49,0	30,34		NW	1	Fine.
26	7	0	43,0	47,0	30,32		NW	1	Fine.
	2	0	53,0	53,0	30,32		NW	2	Cloudy.
27	7	0	47,5	48,5	30,28		NNE	1	Fine.
	2	0	51,0	50,5	30,22		N by W	1	Fine.
28	7	0	42,0	42,0	30,09		N by E	1	Fine.
	2	0	57,0	54,5	29,96		N by E	1	Fine.
29	7	0	47,0	45,5	29,91		NNE	1	Fine.
	2	0	59,0	55,5	29,89		NNE	1	Fine.
30	7	0	44,0	46,5	29,92		NE	1	Cloudy.
	2	0	61,5	57,0	29,87		NNE	1	Cloudy.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
May 1	7	0	48,0	52,5	29,96		NNW	1	Cloudy.
	2	0	61,0	68,5	29,96		NE	1	Fine.
2	7	0	46,0	49,5	30,05		SE	1	Fine.
	2	0	62,0	59,0	30,05		SE	1	Fine.
3	7	0	48,5	49,0	30,01		SE	1	Fine.
	2	0	54,5	55,0	29,98		ESE	1	Rainy.
4	7	0	46,5	50,0	29,93	0,059	NE	1	Cloudy.
	2	0	53,0	56,0	29,93		NE	1	Rainy.
5	7	0	43,5	46,5	30,04	0,040	NE	1	Cloudy.
	2	0	52,0	51,5	30,06		NE	1	Fine.
6	7	0	43,0	44,0	30,17		ESE	1	Fine.
	2	0	51,0	50,0	30,16		ENE	1	Cloudy.
7	7	0	43,5	44,0	30,14		ENE	1	Fine.
	2	0	52,0	49,0	30,12		SE	1	Fine.
8	7	0	42,0	44,0	29,97		NE	1	Cloudy.
	2	0	51,0	49,5	29,88		ESE	1	Fine.
9	7	0	43,0	47,0	29,72		NNW	1	Fine.
	2	0	56,0	51,0	29,68		NE	1	Fine.
10	7	0	42,5	46,0	29,66		NE	1	Fine.
	2	0	59,0	51,0	29,58		ESE	1	Fine.
11	7	0	44,5	49,0	29,62	0,240	ESE	1	Rainy.
	2	0	63,0	67,5	29,73		SSW	1	Cloudy.
12	7	0	58,5	55,5	30,01		SE	1	Fine.
	2	0	72,0	64,0	30,02		SE	1	Fine.
13	7	0	62,0	61,0	29,91		NW	1	Cloudy.
	2	0	73,5	69,5	29,92		NW	1	Fine.
14	7	0	56,0	63,0	29,89	0,041	ESE	1	Cloudy.
	2	0	71,5	68,5	29,89		SSE	1	Fine.
15	7	0	56,5	51,5	29,93		NNW	1	Cloudy.
	2	0	67,0	68,0	29,96		NW	1	Fine.
16	7	0	52,0	54,0	29,96		SE	1	Cloudy.
	2	0	57,0	61,0	30,00		NE	1	Cloudy.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
May 17	7	0	50,0	57,0	30,09	0,050	NE	1	Cloudy.
	2	0	57,0	59,5	30,08		NE	1	Cloudy.
18	7	0	48,5	53,5	29,95	0,114	ENE	1	Rainy.
	2	0	57,5	58,0	29,95		NE	1	Rainy.
19	7	0	55,0	56,0	29,93	0,076	NNW	1	Cloudy.
	2	0	60,5	60,5	29,93		ENE	1	Cloudy.
20	7	0	57,0	57,0	29,89	0,099	ENE	1	Cloudy.
	2	0	64,5	62,0	29,93		NE	1	Fine.
21	7	0	57,0	57,0	30,05		NE	1	Fine.
	2	0	69,0	67,0	30,08		E by N	1	Fine.
22	7	0	50,0	54,0	30,29		NE	1	Cloudy.
	2	0	57,5	58,5	30,29		NE	2	Fine.
23	7	0	57,0	48,0	30,35		NE	2	Fine.
	2	0	58,0	57,0	30,35		NE	2	Fine.
24	7	0	49,0	49,0	30,36		NE	2	Fine.
	2	0	56,0	55,0	30,34		SE	1	Fine.
25	7	0	51,5	50,0	30,23		SW	1	Fine.
	2	0	61,0	58,0	30,22		SW	1	Fine.
26	7	0	50,5	52,0	30,16		ESE	1	Fine.
	2	0	65,0	61,0	30,14		ESE	1	Fine.
27	7	0	48,5	51,0	30,15		ESE	1	Fine.
	2	0	70,0	63,0	30,15		S by W	1	Fine.
28	7	0	53,0	56,5	30,22		SW	1	Fine.
	2	0	72,0	63,5	30,22		SW	1	Fine.
29	7	0	55,5	57,5	30,26		ESE	1	Fine.
	2	0	76,0	70,0	30,16		SE	1	Fine.
30	7	0	60,5	65,0	29,98		SE	1	Fine.
	2	0	80,5	71,5	29,96		SE	1	Fine.
31	7	0	63,0	69,0	30,12		SE	1	Fine.
	2	0	78,0	76,0	30,04		SE	1	Fine.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
June 1	7	0	65,0	66,0	30,04		NE	1	Fine.
	2	0	83,5	78,5	30,03		SW	1	Fine.
2	7	0	67,0	71,0	29,98		SW	1	Fine.
	2	0	84,0	78,0	29,96		SW	1	Fine.
3	7	0	60,0	65,0	29,87		SW	1	Rainy.
	2	0	75,0	74,0	29,80		SSW	2	Showery.
4	7	0	58,5	67,5	29,82	0,169	WSW	2	Cloudy.
	2	0	70,5	70,0	29,79		SW	2	Showery.
5	7	0	60,0	55,0	29,82		S by W	1	Fair.
	2	0	75,0	68,0	29,69		SW	1	Fine.
6	7	0	59,5	56,0	29,60	0,226	SSE	1	Cloudy.
	2	0	68,0	68,5	29,54		W by S	1	Fine.
7	7	0	58,5	65,0	29,55		SSW	1	Cloudy.
	2	0	64,5	67,0	29,55		SW	1	Fair.
8	7	0	53,5	60,0	29,49	0,055	SW	1	Rainy.
	2	0	60,0	62,0	29,42		SW	1	Showery.
9	7	0	57,0	60,0	29,68		SW	1	Fine.
	2	0	64,0	64,0	29,72		SW	1	Showery.
10	7	0	58,5	59,5	29,72	0,064	E by N	1	Fine.
	2	0	60,0	65 0	29,79		SE	1	Showery.
11	7	0	59,5	61,0	29,84		SSW	1	Cloudy.
	2	0	70,5	67,0	29,79		SSE	1	Cloudy.
12	7	0	56,0	63,0	29,82	0,057	NE	1	Rainy.
	2	0	67,0	66,5	29,78		NE	1	Fine.
13	7	0	59,5	64,0	29,77		SE	1	Cloudy.
	2	0	73,5	68,5	29,74		SSE	1	Fine.
14	7	0	63,0	65,0	29,71		SSE	1	Cloudy.
	2	0	66,0	67,0	29,66		SW	1	Fair.
15	7	0	59,5	61,0	29,79		SSE	1	Fine.
	2	0	75,0	68,5	29,65		SSE	1	Fine.
16	7	0	61,0	61,5	29,78		SSW	1	Fine.
	2	0	70,5	69,0	29,78		SSE	1	Fine.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
June 17	7	0	61,5	65,0	29,84		SW	1	Fine.
	2	0	71,0	68,5	29,84		SSW	1	Fair.
18	7	0	65,5	66,0	29,91		SSE	1	Cloudy.
	2	0	73,5	70,5	29,95		SSE	1	Fair.
19	7	0	64,5	67,0	29,91	0,019	ESE	1	Rainy.
	2	0	75,0	72,5	29,84		E by S	1	Showery.
20	7	0	70,0	71,5	29,82	0,098	NE	1	Fine.
	2	0	80,0	79,5	29,84		ESE	1	Fine.
21	7	0	68,0	70,5	29,79		ESE	1	Fine.
	2	0	79,0	76,0	29,70		ESE	1	Fair.
22	7	0	57,5	69,0	29,76		NE	1	Cloudy.
	2	0	75,0	73,0	29,74		NE	1	Fine.
23	7	0	57,0	64,0	29,82		NE	1	Cloudy.
	2	0	62,5	65,0	29,83		N by E	1	Rainy.
24	7	0	55,0	60,0	29,87		NE	1	Cloudy.
	2	0	67,0	66,0	29,85		NE	1	Fair.
25	7	0	55,0	61,0	29,82		NE	1	Cloudy.
	2	0	63,0	64,0	29,80		SSE	1	Cloudy.
26	7	0	59,0	62,0	29,93		NW	1	Cloudy.
	2	0	70,0	68,0	29,97		NW	1	Fine.
27	7	0	60,0	60,0	30,05		NW	1	Fine.
	2	0	73,0	70,0	30,05		NW	1	Fine.
28	7	0	63,5	66,5	30,14		NW	1	Fine.
	2	0	74,0	71,0	30,19		NNW	1	Fine.
29	7	0	59,0	59,0	30,42		NW	1	Fine.
	2	0	77,0	71,5	30,39		NW	1	Fine.
30	7	0	66,0	69,0	30,24		SW	1	Fine.
	2	0	81,0	78,0	30,24		WSW	1	Fine.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
July 1	7	0	65,0	67,0	30,04		S by E	1	Fine.
	2	0	85,0	75,0	29,90		SSW	1	Fine.
2	7	0	74,0	75,0	29,69		SSW	1	Fine.
	2	0	76,0	76,0	29,77		SSW	1	Fine.
3	7	0	65,0	69,0	29,82		SSW	1	Cloudy.
	2	0	74,0	73,0	29,85		SSW	1	Fine.
4	7	0	63,0	65,0	30,04		SSW	1	Fine.
	2	0	71,5	71,0	30,11		WSW	2	Fine.
5	7	0	61,0	62,0	30,25		SSE	1	Fine.
	2	0	74,5	71,5	30,22		SW	1	Fine.
6	7	0	65,0	65,0	30,03		SE	1	Cloudy.
	2	0	78,0	74,5	29,95		SE	1	Fine.
7	7	0	62,0	68,0	29,81	0,050	SE	1	Cloudy.
	2	0	73,0	71,0	29,78		SE	1	Cloudy.
8	7	0	60,0	61,0	29,71	0,132	SE	2	Cloudy.
	2	0	65,5	67,5	29,74		S by W	2	Rainy.
9	7	0	60,5	66,0	29,92	0,293	SW	1	Rainy.
	2	0	68,5	68,0	29,92		NW	2	Cloudy.
10	7	0	68,0	65,0	29,97	0,114	SSW	1	Rainy.
	2	0	69,0	67,0	29,88		SW	1	Cloudy.
11	7	0	59,0	60,5	29,86	0,243	WSW	1	Cloudy.
	2	0	68,0	67,5	29,93		NW	1	Cloudy.
12	7	0	64,5	67,0	30,07		SW	1	Cloudy.
	2	0	76,5	71,0	30,13		SW	1	Fine.
13	7	0	67,0	70,0	30,16		SW	1	Fine.
	2	0	74,5	73,0	30,16		SW	2	Cloudy.
14	7	0	62,0	67,0	30,03		SW	1	Cloudy.
	2	0	66,5	70,0	30,01		WSW	1	Rainy.
15	7	0	57,0	66,0	30,11		WSW	1	Fine.
	2	0	68,0	67,5	30,13		W by S	1	Fine.
16	7	0	58,5	64,0	30,29		WSW	1	Fine.
	2	0	73,0	69,0	30,33		NW	1	Fine.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
July 17	7	0	59,0	53,5	30,33		NW	1	Fine.
	2	0	73,0	69,0	30,29		NW	1	Fine.
18	7	0	62,5	67,0	30,26		SW	1	Fine.
	2	0	73,5	70,5	30,24		SW	1	Fine.
19	7	0	62,0	66,0	30,34		NW	1	Fine.
	2	0	70,0	69,0	30,35		NW	1	Fine.
20	7	0	62,0	65,0	30,41		NW	1	Fine.
	2	0	69,0	68,5	30,41		NW	1	Fine.
21	7	0	61,0	64,0	30,44		ESE	1	Fine.
	2	0	72,0	70,0	30,41		NW	1	Fine.
22	7	0	61,0	63,0	30,33		NW	1	Fine.
	2	0	75,0	72,0	30,23		NW	1	Fine.
23	7	0	62,0	65,0	30,17		NNW	1	Cloudy.
	2	0	72,5	71,0	30,15		NW	1	Fine.
24	7	0	64,0	69,0	30,07		SSW	1	Cloudy.
	2	0	79,0	75,0	30,03		WSW	1	Fine.
25	7	0	65,0	70,0	29,96		WSW	1	Fine.
	2	0	77,5	74,0	29,93		SSW	1	Fine.
26	7	0	60,0	65,0	29,89		SW	1	Cloudy.
	2	0	71,0	70,0	30,01		SW	1	Cloudy.
27	7	0	59,0	67,0	30,06		NNW	1	Cloudy.
	2	0	73,0	70,0	30,06		NNW	1	Cloudy.
28	7	0	73,0	67,0	30,05		NW	1	Fine.
	2	0	75,0	71,0	30,01		SW	1	Fine.
29	7	0	66,0	68,0	30,01	0,213	SW	1	Cloudy.
	2	0	76,0	73,0	30,08		SW	1	Fine.
30	7	0	68,0	71,0	30,19		SW	1	Cloudy.
	2	0	80,0	76,0	30,17		SW	1	Fine.
31	7	0	67,0	70,0	30,06		SE	1	Fine.
	2	0	84,0	80,0	30,05		SE	1	Fine.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Aug. 1	7	0	68,5	71,0	29,96		WSW	1	Cloudy.
	2	0	78,0	75,0	30,01		SW	1	Fine.
2	7	0	63,0	65,0	30,12		SW	1	Fine.
	2	0	71,5	69,5	30,11		WSW	1	Fine.
3	7	0	63,0	64,0	30,27		SW	1	Fine.
	2	0	73,0	70,0	30,28		SW	1	Fine.
4	7	0	63,0	65,0	30,39		SW	1	Cloudy.
	2	0	73,0	70,0	30,37		NE	1	Fine.
5	7	0	62,9	64,0	30,33		NE	1	Fine.
	2	0	72,0	70,0	30,24		NE	1	Fine.
6	7	0	63,0	65,5	30,07		SSE	1	Cloudy.
	2	0	77,0	72,0	30,07		SE	1	Fine.
7	7	0	63,0	66,0	30,08		SE	1	Cloudy.
	2	0	77,0	72,0	30,07		SE	1	Fine.
8	7	0	62,0	68,0	29,94	0,026	WSW	1	Cloudy.
	2	0	66,0	68,5	29,91		SW	1	Rainy.
9	7	0	63,0	67,0	29,95	0,684	SSW	1	Fair.
	2	0	78,0	71,5	29,98		SSW	1	Fine.
10	7	0	68,5	70,5	29,90	0,042	SSW	1	Cloudy.
	2	0	80,0	76,0	30,03		SSW	1	Cloudy.
11	7	0	63,0	71,5	30,06		SSW	1	Fine.
	2	0	82,0	77,0	30,05		SE	1	Fine.
12	7	0	68,0	71,0	30,02		SSW	1	Fine.
	2	0	81,5	78,0	30,01		SW	1	Fine.
13	7	0	67,0	72,0	29,93		SW	1	Rainy.
	2	0	80,5	75,5	29,87		SW	1	Fine.
14	7	0	63,0	72,5	29 93		SW	1	Fine.
	2	0	75,0	74,0	29,85		SW	1	Cloudy.
15	7	0	58,0	68,5	29,65	0,271	SSW	2	Fair.
	2	0	65,5	70,0	29,59		SSW	2	Rainy.
16	7	0	61,0	67,5	29,62	0,067	SW	1	Fine.
	2	0	72,0	69,5	29,61		SSW	1	Fair.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
					Inches.	Inch.	Points.	Str.	
Aug. 17	7	0	60,5	65,5	29,74	0,182	SW	1	Fine.
	2	0	72,0	68,0	29,83		SW	1	Fine.
18	7	0	57,0	64,0	29,89	0,087	SW	1	Fine.
	2	0	73,0	68,5	29,86		SSW	1	Fine.
19	7	0	58,0	64,5	29,73		NNE	1	Rainy.
	2	0	59,0	65,0	29,66		N by W	1	Rainy.
20	7	0	53,0	62,5	29,77	0,147	NW	1	Fine.
	2	0	67,0	66,0	29,82		NW	1	Cloudy.
21	7	0	55,0	62,0	30,11		NW	1	Fine.
	2	0	68,0	66,0	30,15		NNW	1	Fine.
22	7	0	52,0	61,0	30,25		NE	1	Fine.
	2	0	71,0	69,5	30,27		NE	1	Fine.
23	7	0	60,0	65,0	30,04		SSW	1	Cloudy.
	2	0	80,0	70,0	29,96		SW	1	Fine.
24	7	0	63,0	67,0	29,76		SE	1	Cloudy.
	2	0	75,5	71,0	29,66		SSE	1	Fine.
25	7	0	61,5	68,5	29,54	0,302	WSW	1	Fine.
	2	0	72,0	71,0	29,67		SW	1	Cloudy.
26	7	0	61,5	68,0	29,93		SW	1	Fine.
	2	0	75,5	73,0	29,96		SW	1	Cloudy.
27	7	0	68,0	69,5	29,82		SSE	1	Cloudy.
	2	0	79,0	76,0	29,75		SSW	1	Fine.
28	7	0	67,5	70,5	29,66		SSW	1	Cloudy.
	2	0	65,0	72,5	29,58		SW	1	Rainy.
29	7	0	64,0	70,0	29,59		SW	2	Fine.
	2	0	72,5	71,0	29,77		SW	2	Fine.
30	7	0	64,0	68,0	30,01		SW	1	Fine.
	2	0	74,0	70,0	29,98		SSE	1	Cloudy.
31	7	0	66,0	70,0	29,72	0,390	SE	1	Rainy.
	2	0	75,0	73,0	29,88		SE	1	Fine.

1781	Thermometer without.			Thermometer within.			Barometer.			Rain.
	Greateſt height.	Leaſt height.	Mean height.	Greateſt height.	Leaſt height.	Mean height.	Greateſt height.	Leaſt height.	Mean height.	Inches.
Jan.	51,5	25,0	38,5	49,0	32,0	35,9	30,55	29,04	29,90	1,348
Feb.	52,5	31,0	42,8	51,0	36,0	45,7	30,34	28,95	29,65	1,676
Mar.	60,5	32,0	44,8	53,5	35,0	45,9	30,47	29,88	30,21	0,292
Apr.	66,5	37,0	49,2	67,0	38,0	52,8	30,34	29,29	29,88	0,650
May	80,5	42,0	56,8	76,0	44,0	56,8	30,36	29,58	30,05	0,619
June	84,0	53,5	66,2	79,5	55,0	66,8	30,42	29.42	29,84	0,688
July	84,0	57,0	68,4	80,0	53,5	68,7	30,44	29,69	30,05	1,045
Aug.	82,0	52,0	67,7	78.0	61,0	69,2	30,39	29,54	29,95	2,198
Mean of 8 mon.			53,0			55,2			29,95	8,516

APPENDIX.

I. *Account of a new Kind of Rain. Written by the Count* de Gioeni, *an Inhabitant of the 3d Region of Mount Etna; communicated by Sir* William Hamilton, *K. B. F. R. S.* See p. 1.

> *Volat per Mare magnum cinis decoctus, et terrenis nubibus excitatis, tranſmarinas quoque provincias pulvereis guttis implevit.* CASSIOD. lib. IV. var. epiſt. 50.

THE morning of the 24th inſtant there appeared here a moſt ſingular phenomenon. Every place, expoſed to the air, was found wet with a coloured cretaceous grey water, which, after evaporating and filtrating away, left every place covered with it to the height of two or three lines; and all the iron-work that was touched by it became ruſty.

The public, inclined to the marvellous, fancied various cauſes of this rain, and began to fear for the animals and vegetables.

In places where rain water was uſed, they abſtained from it: ſome ſuſpecting vitriolic principles to be mixed with it, and others predicting ſome epidemical diſorder.

Thoſe who had obſerved the exploſions of Etna twenty days and more before, were inclined to believe it originated from one of them.

The ſhower extended from N. ¼ N.E. to S. ¼ SW. over the fields, about ſeventy miles in a right line from the vertex of Etna.

There is nothing new in volcano's having thrown up ſand *, and alſo ſtones †, by the violent expanſive force generated within them, which ſand has been carried by the wind to diſtant regions.

But the colour and ſubtilty of the matter occaſioned doubts concerning its origin; which increaſed from the remarkable circumſtance of the water in which it came incorporated ‡; for which reaſons ſome other principle or origin was ſuſpected.

It became, therefore, neceſſary by all means to aſcertain the nature of this matter, in order to be convinced of its origin, and of the effects it might produce. This could not be done without the help of a chemical analyſis. To do this then with certainty, I endeavoured to collect this rain from places where it was moſt probable no heterogeneous matter would be mixed with it. I therefore choſe the plant called *Braſſica Capitata*, which having large and turned-up leaves, they contained enough of this coloured water; many of theſe I emptied into a veſſel, and left the contents to ſettle till the water became clear.

This being ſeparated into another veſſel, I tried it with vegetable alkaline liquors and mineral acids; but could obſerve no decompoſition by either. I then evaporated the water in order to reunite the ſubſtances that might be in ſolution; and touching it again with the aforeſaid liquors, it ſhewed a ſlight effervefcence with the acids. When tried with the ſyrup of violets, this became a pale green; ſo that I was perſuaded it contained a calcareous ſalt*. With the decoction of galls no precipitation was produced.

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* The authority of CASSIOD. prefixed to this account is ſtrengthened by SENECA, in his 2 *lib. de Queſt. Nat:*

Ætna aliquando multo igne abundavit, ingentem vim arenæ urentis effudit, involutus eſt dies pulvere, populoſque ſubita nox terruit.

But without having recourſe to the numerous old accounts of this volcano, and of Veſuvius, we have, within theſe twenty years, ſeen many of thoſe rains in Sicily originating in Etna; and the laſt, preceding the eruption of laſt year, was compoſed of little fragments of bituminous pumice ſtone, or *ſtumie*.

† The ſtone, deſcribed by PLINY, which fell in Thrace, the ſhower of ſtones on mount Albano after the ruin of Alba, which LIVY mentions, and many others of like nature, remarked by the ancients as miraculous rains, have been diſcovered to be volcanic. As to Etna we have, in our days, ſeen new mountains formed by the ſtones, or rather the lava; and as to the ancients, beſides STRABO and many others, the poet PINDAR writes, that *aliquando non tantum rivos igneos ejecit, ſed ſaxa ignita.* PIND. ap Brit. lib. V. c. 14 p. 2.

‡ In many of the writers on Etna, ſhowers of ſand, or other productions, mixed with water, are not to be found.

The matter being afterwards dried in the ſhade, it appeared a very ſubtile, fine earth, of a cretaceous colour, but inert, from having been diluted by the rain.

I next thought of calcining it with a ſlow fire, and it aſſumed the colour of a brick. A portion of this being put into a crucible, I applied to it a ſtronger heat, by which it loſt almoſt all its acquired colour. Again, I expoſed a portion of this for a longer time to a very violent heat (from which a vitrification might be expected); it remained however quite ſoft, and was eaſily bruiſed, but returned to its original duſky colour.

From the moſt accurate obſervations of the ſmoke from the three calcinations, I could not diſcover either colour or ſmell that indicated any arſenical or ſulphureous mixture.

Having therefore calcined this matter in three portions, with three different degrees of fire, I preſented a good magnet to each; it did not act either on the firſt or ſecond; a ſlight attraction was viſible in many places on the third: this perſuaded me, that this earth contains a martial principle in a metallic form, and not in a vitriolic ſubſtance †.

The nature of theſe ſubſtances then being diſcovered, their volcanic origin appears; for iron, the more it is expoſed to violent calcination, the more it is divided, by the loſs of its phlogiſtic principle; which cannot naturally happen but in the great chimney of a volcano. Calcareous ſalt, being a marine ſalt combined with a calcareous ſubſtance by means

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* Tried likewiſe with a ſolution of lead in the vegetable acid, it loſt its natural colourand its tranſparency, and became milky. I ſhould be inclined to believe this to be the effect of the alkaline particles, and thus account for the efflorefcence on the iron's being expoſed to the air.

† Becauſe, otherwiſe the water would not have produced an effervefcence with the acids, but would have ſhewn it with the alkalies; and, in the triple calcination, the red colour would rather have been increaſed than diminiſhed.

of violent heat *, cannot be otherwiſe compoſed than in a volcano †.

As to their dreaded effects on animals and vegetables, every one knows the advantageous uſe, in medicine, both of the one and the other, and this in the ſame form as they are thus prepared in the great laboratory of nature.

Vegetables, even in flower, do not appear in the leaſt macerated, which has formerly happened from only ſhowers of ſand ‡.

How this volcanic production came to be mixed with water may be conceived in various ways.

Etna, about its middle regions, is generally ſurrounded with clouds that do not always riſe above its ſummit, which is 2900 paces § above the level of the ſea. This matter being thrown out, and deſcending upon the clouds below it, may happen to mix and fall in rain with them in the uſual way. It may alſo be conjectured, that the thick ſmoke which the volcanic matter contained might, by its rarefaction, be carried in the atmoſphere by the winds, over that tract of country ||; and then, cooling ſo as to condenſe and become ſpecifically

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* The burning of lime-ſtone may indeed produce the compoſition from whence reſults the calcareous ſalt: but it is evident, that ſuch a quantity could only proceed from a volcano.

† Many and repeated experiments on the produce of Etna have perſuaded me, that marine ſalt is one of the chief and moſt abundant *menſtrua* which excite the effervescence of a volcano, or that it is the baſis of it (as a friend of great knowledge has lately made me believe). I find calcareous ſalt in the old lava, and common ſalt ſublimated to ammoniac in the fiſſures and openings of the new irruptions. But this is not the place for that which requires a larger volume. I may, perhaps, ſay more of it on another occaſion.

‡ I have repeatedly obſerved, that the ſand-ſhowers of our mountain are moſtly compoſed of calcined matter, and of little cryſtals of ſchorl, with a ſmall portion of arſenical and ſometimes ſaline ſulphureous particles, which unites the ſchorl to the other ſubſtances, ſo that the particles or grains are thereby enlarged. Sometimes the rain falls to the ground ſtill warm.

§ The meaſure of the height of the mountain has twice come out to be thus deſcribed; not, however, that I give it for certain, well knowing that altimetry requires exact inſtruments and repeated obſervations. I mean to try it with the barometer, when convenient.

|| That this hypotheſis may not appear exaggerated as to the quantity of ſmoke that

ſpecifically heavier than the air, might deſcend in that coloured rain.

I muſt, however, leave to philoſophers (to whom the knowledge of natural agents belongs) the examination and explanation of ſuch phenomena, confining myſelf to obſervation and chemical experiments *.

P. S. On Friday the 4th of May, about a quarter paſt three in the afternoon, a ſlight ſhock of an earthquake was felt in the country about Etna, which became more ſenſible at ſome diſtance from the mountain: its direction was from north to ſouth. The volcano had continued its flames and exploſions; and the night before, a column of ſmoke, compoſed of globes as it were piled upon each other, had aſcended over the crater to double the height of the mountain, as far at leaſt as one could judge at the diſtance of twenty-two miles, which the vertex is in a right line from this city. This remained the whole night perpendicular, only one of the globes had ſeparated and lengthened out to the weſtward from the ſummit. Now and then all the inſide of the column, and of the lengthened outpart, became illuminated by electric fire, which was of a deep red colour, and gradually went out again, beginning at the bottom, in about two ſeconds.

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that muſt be ſuppoſed, I ſhall mention what was obſerved by CICERO: *Cratere flamma erumpit, fumo mixta tam copioſo, ut, dum Boreas ſpirat, Melitam uſque per aera illum ſublimem propellat ad* IX. *millia paſſuum ſpatium.* CIC. de Nat. Deor. lib. II.

* WALLERIUS (in his Mineral. vol. II. Hidrol. § 5.) ſays: *La Phyſique eſt plus univerſelle dans ſes vûes, et plus philoſophique dans ſon examen, le phyſicien enviſage, raiſonne, explique, le naturaliſte regarde, ramaſſe, et range; celui-ci vous dira il exiſte tel corps dans la nature, il eſt fait, ſoit au dedans, ſoit au debors de telle ou telle maniere, il eſt de tel ou tel regne, claſſe, ordre, eſpéce, variété; celui la pretendra vous expliquer les cauſes de ſon exiſtence, de ſes formes, et de ſes propriétés.*

The illuſtrious LINNÆUS, in Anal. Tranſalp. anno 1740, § 2. ſays thus: *Phyſica eſt ſcientia de qualitatibus elementorum, hiſtoria naturalis autem circa cognitionem corporum naturalium verſatur.* The true naturaliſt ought to be learned both in phyſics and in chemiſtry; but ſtill we know not where the diviſion between the two ſciences is.

The fire has continued on the crater till this day, May 8th, ejecting red-hot maſſes or ſtones, which rolling beautifully down the cone, have illuminated this region; ſome lava has run over from the crater towards the W.N.W. but without having force enough to burſt the ſides or walls of the volcano; ſo that we may apply the hiſtorical paſſage, MARCO ÆMILIO C. AURELIO Coſſ. *Ætna mons terræmotu, ignes ſuper verticem late diffudit*. Jul. Obſequ. de Prodig. c. 89.

II. *Of the Method of rendering very ſenſible the weakeſt Natural or Artificial Electricity. By Mr.* Alexander Volta, *Profeſſor of Experimental Philoſophy in* Como, *&c. &c.; communicated by the Right Hon.* George *Earl* Cowper, *F. R. S.* See p. 237.

PART I.

1. IT will be readily allowed, that an apparatus capable of rendering perceptible, or, as it were, of magnifying the ſmalleſt, and otherwiſe unobſervable, degrees of natural as well as artificial electricity, is of great advantage to the ſcience of electricity in general, and eſpecially for the inveſtigation of atmoſpherical electricity, which by this means may be rendered very ſenſible and conſpicuous when it is not to be diſcovered by common atmoſpherical conductors. This method is founded upon a particular uſe of my *electrophorus*, which is a machine well known to electricians.

2. Whenever in obſerving the atmoſpherical electricity no degree of it can be diſcovered by the ordinary methods of performing thoſe experiments, it is difficult to determine whether any electricity at all does or does not exiſt in the atmoſphere at thoſe times; ſince it may exiſt, and the quantity of it only be ſo ſmall as not to affect the electrometers employed. An ordinary conductor, erected in the beſt manner for the purpoſe of obſerving the atmoſpherical electricity, when the ſky is free from electrical clouds, ſeldom or never ſhews any ſigns of electricity. In that caſe, therefore, if we rely upon the common electrometers, even the moſt ſenſible, we muſt conclude, that neither the conductor nor the atmoſphere, ſo high as the conductor reaches, contains any electricity; but by means of the apparatus I am going to deſcribe, it will be found, that the ſaid conductors are never entirely void of electricity,

tricity, and it muſt be conſequently concluded, that the air, which ſurrounds them, is alſo at all times electrified. This method not only ſhews the exiſtence of electricity, but gives alſo room to aſcertain whether it is poſitive or negative, and that when the atmoſpherical conductor itſelf is not capable of attracting the fineſt thread; but if the conductor were to ſhew any very ſmall attraction, then, by means of our apparatus, there may be obtained even ſtrong ſparks.

3. The electrophorus in this caſe might perhaps better deſerve the name of *electrometer*, or *micro-electrometer*, but I had rather call it a *condenſer of electricity*, for the ſake of uſing a word which expreſſes at once the reaſon and cauſe of the phenomena to be treated of in this paper, as will be made evident in the ſecond part.

The whole method may be reduced to the following few obſervations. I. An electrophorus muſt be procured, the reſinous coat of which muſt be very thin, and either not at all electrified, or, if electrified, its electricity be entirely extinguiſhed.

II. Its uſual metal plate muſt be laid upon this reſinous and unelectrified plate, in full and flat contact; but care muſt be taken that it does in no point touch the lamina of metal upon which the reſinous ſtratum is uſually faſtened.

III. Thoſe plates being ſo conjointly placed, a conducting communication, *viz.* a wire muſt be brought from the atmoſpherical conductor to touch the metal plate of the electrophorus, and to touch that only.

IV. The apparatus muſt be left in that ſituation for a certain time, *viz.* till the metal plate may have acquired a ſufficient quantity of electricity through the conducting communication, which brings it from the atmoſpherical conductor very ſlowly.

V. Laſtly, the conducting communication muſt be removed from the contact of the metal plate: the metal plate is then ſeparated from the reſinous one, by lifting it up by its inſulating handle, after which it is in a ſtate of attracting, of electrifying an electrometer, or, if the electricity is ſufficiently ſtrong, of giving ſparks, &c. at the ſame time the atmoſpherical conductor itſelf ſhews either no electricity at all, or exceeding ſmall ſigns of it.

4. It was mentioned above (IV.) that the conducting wire muſt be left in contact with the metal plate *for a certain time*, the length of which, however, is not eaſily determined, ſince it depends upon variable circumſtances. When the conductor itſelf ſhews no ſigns of electricity, then it will be neceſſary to leave the apparatus, as directed above, during eight, ten, or more minutes. But if the conductor itſelf is capable of juſt attracting a very ſmall thread, then it will be ſufficient to leave the apparatus in contact as above mentioned, for a few ſeconds only, in order afterwards to obtain from it very conſpicuous electrical appearances.

5. Reſpecting the conducting communication between the atmoſpherical conductor and the metal plate, care ſhould be taken that it be made of the feweſt joints poſſible, or rather of one piece, ſince the difficulty of tranſmitting very ſmall quantities of electricity is conſiderably increaſed by every interruption, and it may thereby be quite obſtructed, as is often the caſe when a chain is uſed for that purpoſe.

6. As for the electrophorus to be uſed, it muſt be farther remarked, firſt, that its being very thin, as mentioned above, is of great importance; it having been obſerved, that the thinner the reſinous ſtratum is, the greater quantity of electricity can be accumulated into the metal plate laid upon it; which is the caſe whether the electricity is brought to it from the atmoſphere, as in the abovementioned inſtance, or from any other electric power. The thickneſs of one-fiftieth of an inch, or that of a common coat of varniſh, is very proper; whereas if the reſin was an inch thick or more, the experiments would anſwer very badly.

7. Secondly, the ſurface of the reſinous ſtratum, as well as the under ſurface of the metal plate, muſt be as plain and as ſmooth as poſſible, in order that the two ſurfaces may coincide more perfectly when laid one upon the other. It is well known how much this circumſtance favours the effect of the electrophorus; for this reaſon, in my publication on that inſtrument, I recommended

I recommended it as a thing eſſential to obſerve [*]: but this circumſtance is ſtill more eſſential when the ſame apparatus is to ſerve as a *condenſer* of electricity.

8. Laſtly, it deſerves to be repeatedly and particularly obſerved, that the reſinous plate, when it is to be uſed for our experiment, ſhould be quite free from any the leaſt electricity, otherwiſe the experiments cannot be depended upon. If, therefore, the reſinous plate has been excited before, ſo as to remain in ſome meaſure electrified, all poſſible care ſhould be taken to deprive it of that electricity, which however is not eaſily done. The moſt effectual method of doing it to expoſe the reſinous plate to the hot rays of the ſun or to the fire, ſo that its ſurface may be ſlightly melted, by which means it will entirely loſe its electricity [†]. The flame of a candle, or of a piece of paper, will eaſily deprive the reſin of its electricity, if its ſurface be paſſed over the flame. In order to obſerve whether the reſinous plate is quite free from any electricity, the metal plate muſt be laid upon it, there it muſt be touched with a finger, and afterwards, being lifted up after the uſual manner, it muſt be preſented to a fine hair; for if the hair is not attracted, you may conclude, that the reſinous plate has no electricity, and conſequently the apparatus is fit to be uſed as a *condenſer* of electricity.

9. Were I aſked, to what degree the electricity might be condenſed, or how much the electrical phenomena could be

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[*] See the two letters addreſſed to Dr. PRIESTLEY, and publiſhed in the *Scelta d'Opuſculi intereſſanti* of Milan for the year 1775.

[†] It has been believed for a long time, that to heat, and eſpecially to melt, ſulphur and reſins, was ſufficient to excite in them ſome electricity; but except the tourmalin and ſome other ſtones, which are really excited by heat alone, the reſins and ſulphur never become electrified by that means, except when they have by ſome means or other ſuffered any friction. The miſtake, as Father BECCARIA obſerved, was occaſioned by this, *viz.* that even the leaſt friction of the hand, or other body, is ſufficient to excite ſuch ſubſtances in thoſe favourable circumſtances; without which friction, thoſe ſubſtances, melted and left to cool by themſelves, are ſo far from acquiring any electricity, that they loſe every veſtige of it in caſe they were excited before the fuſion, as may be eaſily proved by experiment: nor ought this to appear wonderful, ſince fuſion or a ſtrong degree of heat renders every body a conductor of electricity.

increaſed by this apparatus; I would anſwer, that it is not eaſy to be determined, as it depends upon various circumſtances; however, *cæteris paribus*, the augmentation is greater in proportion as the body which ſupplies the metal plate with the electricity has a greater capacity, and is larger in proportion as the electricity is weaker. Thus we obſerved above (§ 2. and following) that if the atmoſpherical conductor has barely power enough to attract a very fine thread, it is neverthelefs capable of infuſing ſuch a quantity of electricity into the metal plate of the electrophorus, as to let it not only actuate an electrometer, but even dart ſtrong ſparks. But if the electricity of the atmoſpherical conductor is ſo ſtrong as to afford ſome ſparks, or to let the index of the electrometer riſe to five or ſix degrees, then the metal plate of the electrophorus, which receives the electricity from this conductor, according to our method, will certainly let the index of the electrophorus riſe to the higheſt degree, and will give a ſtronger ſpark, yet it may be plainly perceived, that the condenſation is proportionably leſs in this than in the other caſe. The reaſon is, becauſe the electricity cannot be accumulated beyond the greateſt degree, *viz.* when the electricity is increaſed ſo much as to be diſſipated every way. Therefore, according as the electric power, which ſupplies the condenſer, is neareſt to the higheſt degree, the condenſation is proportionably leſs: but in that caſe there is no need of a condenſer, ſince its principal uſe is to collect and render ſenſible that ſmall quantity of electricity, which would otherwiſe remain imperceptible and unobſerved.

10. Whenever, therefore, the atmoſpherical conductor by itſelf gives ſufficiently ſtrong ſigns of electricity, then there is no occaſion to uſe our condenſing apparatus. Beſides, when the electricity is ſtrong, it often happens, that part of the electricity of the metal plate is impreſſed upon the reſin, in which caſe the apparatus acts as an electrophorus, and conſequently is unfit for our purpoſe (§ 8.).

11. In order to avoid ſuch an inconvenience, I have thought of ſubſtituting to the reſinous plate a plane, which ſhould not be a perfect electric, or quite impervious to electricity, but which ſhould be an imperfect conductor, ſuch as might hinder, in a certain

tain meaſure only, the free paſſage of the electric fluid through its ſubſtance. There are many conductors of this kind; as, for inſtance, a clean and dry marble ſlab, a plate of wood (likewiſe clean and very dry, or covered with a coat of varniſh, or wax) and the like. The ſurface of thoſe bodies does not contract any electricity, or if any electricity adheres to them, it vaniſhes ſoon, on account of their ſemi-conducting nature; for which reaſon they cannot anſwer the office of an electrophorus, and therefore are more fit to be uſed as condenſers of electricity.

12. Beſides the advantages above mentioned, there is another, which ariſes from ſubſtituting an imperfectly conducting plane to the reſinous plate, namely, that the metal plate laid upon one of theſe does actually condenſe or acquire a greater quantity of electricity than when laid upon the reſinous plate, or other perfect electric; for ſince, as was ſaid above, § 6. the thinner the reſinous ſtratum is, the better it anſwers our purpoſe; in the caſe of a varniſhed or waxed board, this ſtratum becomes exceedingly thin, and it becomes nothing when an imperfectly conducting ſubſtance is uſed, ſuch as a marble ſlab, a very dry piece of wood, &c.

13. On the other hand, care ſhould be taken, in chooſing the above mentioned plane, that it be not too much of a conducting nature, or capable of becoming ſo in a very ſhort time, it being quite neceſſary, that the electricity ſhould find a conſiderable degree of reſiſtance in going through its ſubſtance. In chooſing, or in preparing, ſuch a plane by drying, or otherwiſe, it is better to render it too near to than too far from the nature of a non-conductor. A marble ſlab, or a board properly dried, anſwers admirably well, and is preferable to any other plane: otherwiſe the reſinous plate of an electrophorus is preferable to a common table or marble ſlab not prepared; for theſe bodies, being in ſome meaſure imbibed with moiſture, conduct much better than is neceſſary.

14. To be more particular, I ſhall add, that for this purpoſe it is better to uſe a flat piece of marble, and to grind it againſt the metal plate, till they coincide ſo well as to ſhew a ſenſible coheſion between them. Afterwards the piece of marble ſhould be expoſed for ſeveral days to the

heat of a warmed place, ſuch as an oven, a chimney, &c. in order to expel the moiſture, and to render it quite fit for our experiments (§ 12. 13.). The marble, thus prepared, will continue dry for a conſiderable time, except it be long expoſed to very damp air. As for the ſmall quantity of moiſture which the marble may accidentally and ſuperficially attract, it may be removed by expoſing it to the ſun, or to a fire, or even by wiping it with a dry and clean cloth, previous to the performing of experiments.

15. It is not every ſort of marble that will anſwer equally for this purpoſe. The old marbles, which have been long preſerved in dry places, anſwer better than thoſe which have been recently dug from the quarry. The difference of the ſpecies of marble is alſo of conſequence in this buſineſs; I have found ſome marbles which, without any preparation, anſwer vaſtly well, whereas others will not do near ſo well, even when properly prepared; excepting, however, when they are preſerved hot during the experiment; for, in that caſe, they anſwer better than the beſt pieces of marble that are not preſerved hot. It is always advantageous to warm the marble previous to the experiment.

16. Inſtead of preparing the piece of marble by a long continued heat, it will be ſufficient to give it a coat of copal varniſh, or amber, or lac varniſh: after which it muſt be kept in an oven for a ſhort time. By this means even the worſt ſort of marble anſwers very well, even without previouſly warming or keeping it hot during the experiment.

17. By means of the varniſh even a metal plate may be uſed inſtead of the marble. This ſhould be firſt made flat by grinding it againſt the upper plate, and then it muſt be varniſhed, but rather thicker than when the varniſh is laid upon the marble. In this caſe both the plates might be varniſhed, though it is ſufficient to varniſh one of them.

18. Here it may be ſaid, that in fact we are returned to the electrophorus. This is true; and indeed the varniſhed metal, or marble, or wood, may be excited by a very ſlight friction, even ſometimes by the ſimple laying of the metal plate upon it,

eſpecially when they are hot; hence there is no occaſion to warm them, when they are good for the purpoſe, leſt they ſhould be ſo well prepared as to be eaſily excited, and then act like an electrophorus.

19. However, the advantages which a varniſhed plate has above the common electrophorus are, 1. that the varniſh is always thinner than the common reſinous ſtratum of an electrophorus. 2. That the varniſh acquires a more ſmooth and plain ſurface; hence the metal plate may be more eaſily, and to more advantage, adapted to it.

20. Inſtead of the above mentioned plane of marble or metal varniſhed, there may be ſubſtituted, with equal advantage, any ſort of plane covered with dry and clean oil-cloth or oil-ſilk or ſattin and other ſilk-ſtuff that is not conſiderably thick; which will anſwer very well, without requiring any more than perhaps a ſlight warming. The ſilk-ſtuffs anſwer better for this purpoſe than thoſe made of cotton or wool, and theſe better than linen. However, by a previous drying and keeping them hot during the experiment, paper, leather, wood, ivory, bone, and every ſort of imperfect conductor, may be made to anſwer to a certain degree.

21. If thoſe imperfectly conducting ſubſtances were dried too much, then they would become quite electrics, and conſequently uſeleſs for our purpoſe (as will be made appear better in the ſecond part of this paper), excepting when they were uſed like reſins, &c.

22. I muſt not omit to mention alſo, that the apparatus may be rendered more ſimple by applying the ſilk or other ſemi-conducting ſtratum to the upper, *viz.* to the metal plate, which is furniſhed with a glaſs handle inſtead of the marble or other plate, which in that caſe becomes uſeleſs: for in its ſtead a plane of any kind may be uſed, ſuch as a common wooden or marble table, even not very dry, a piece of metal, a book, or other conductor, whether perfect or imperfect, it being only neceſſary that its ſurface be flat.

In fact, nothing more is requiſite for our experiment than that the electricity, which tends to paſs from one ſurface to the other, ſhould find ſome reſiſtance or oppoſition in either of the ſurfaces, as will be made more evident in the ſecond part. It is immaterial whether the non-conducting or ſemi-conducting ſtratum be laid upon one or the other of the planes, it being only neceſſary that they ſhould coincide very well together, which cannot be eaſily obtained when a common table is uſed for one of the planes, which is the only reaſon why it is better to uſe two planes which have been worked flat by grinding one upon the other, and one of them varniſhed, &c. A ſingle metal plate, covered with ſilk, with three ſilk ſtrings faſtened to it by way of a handle, may be conveniently uſed for ordinary experiments.

23. Hitherto we have conſidered the uſe of our condenſer in exploring the weak atmoſpherical electricity, which is brought down by the atmoſpherical conductor*. But this, though the principal, is not the only uſe to which it may be applied. It ſerves likewiſe to diſcover the artificial electricity when this is ſo weak as not to be diſcoverable by any other means, which happens in various caſes, ſome of which I ſhall now proceed to mention.

24. A Leyden phial charged, and then diſcharged by touching its coated ſides three or four times with the diſcharging rod, or the hand, ſeems to be quite deprived of electricity, yet

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* Here it will be proper to mention a remarkable obſervation, which I have made on the atmoſpherical electricity with the help of the *condenſer*. The late Mr. CANTON and others affirmed that they had obtained ſtronger ſigns of electricity from their atmoſpherical apparatus at the time of an *aurora borealis*, than at other times; but various other philoſophers doubted of the influence of electricity in that meteor, and ſome abſolutely denied it. I myſelf was much in doubt about it; but at preſent Mr. CANTON's aſſertion ſeems to be eſtabliſhed beyond a doubt, as I have obſerved by actual experiment. During the ſtrong *aurora borealis*, which appeared in the night of the 28th of July, 1780, the light of which riſing gradually from the horizon, reached the zenith at near eleven o'clock, and enlightened the heavens with a reddiſh light, the weather being clear and windy; our condenſing apparatus being applied to an atmoſpherical conductor, gave fine bright ſparks; whereas, at other times, that is, in clear weather, and at every hour of the day or night, the ſame apparatus afforded either no ſparks at all, or exceedingly ſmall ones, the reaſon of which was becauſe the ſaid conductor was not much elevated.

if you touch with the knob of it the metal plate of our condenſer, when properly ſituated (*viz.* upon an imperfectly conducting plane, &c.) and immediately after take up the ſaid plate, this will be found to give very conſpicuous ſigns of electricity, which ſhews that the Leyden phial is not quite deprived of electricity as it appeared. But if the phial was left ſo far charged as juſt to attract a light thread, then if the metal plate were to be touched by the knob of it, even for a moment, it would afterwards, when lifted up, give a ſtrong ſpark, and if then it were to be touched again by the knob of the phial, it would afford a ſecond ſpark hardly ſmaller than the former, and thus ſpark after ſpark may be obtained for a long time, which is a very ſurprizing experiment.

This method of producing ſparks by means of a phial, which is not charged ſo high as to give ſparks of itſelf, is very convenient for various pleaſing experiments; as, for inſtance, that of lighting the inflammable air-piſtol, or lamp, contrived by me, eſpecially when a perſon is provided with one of thoſe phials, prepared after the manner recommended by Mr. TIBERIUS CAVALLO *, which when charged may be carried in the pocket for a long time. Thoſe phials, as they retain a ſenſible charge for ſeveral days, will retain an inſenſible one for weeks or months. I mean, by an inſenſible charge, ſuch as cannot be diſcovered but by the help of the condenſer, in which caſe it becomes more than ſenſible, and ſufficient for the experiment of the inflammable air-piſtol, &c.

25. Secondly, Suppoſe you have an electrical machine ſo badly in order that its conductor will not afford any ſpark, but will juſt attract a thread; then if you let this conductor touch the metal plate of the condenſer, and after ſuffering it to continue in that ſituation for a few minutes, whilſt the machine is kept in motion, lift up the metal plate, you will obtain from it a ſtrong ſpark.

26. Thirdly, In caſe the electrical machine acts very well, but its conductor is ſo badly inſulated, that it will not give any ſparks, as when the conductor touches the walls

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* See his Treatiſe on Electricity.

of the room, or when a chain falls from it upon the table; then if you let the ſaid conductor in that ſtate touch the metal plate of the condenſer, whilſt the electrical machine is in action, the plate will afterwards give ſufficiently ſtrong ſigns of electricity, which ſhews the great power this apparatus has of drawing and condenſing the electricity.

27. Fourthly, The uſual way of rubbing divers bodies, and then preſenting them to an electrometer in order to examine their electricity, is often inſufficient, that is, it makes the experimenter believe, that a body has not acquired any electricity at all, only becauſe the quantity of it is too ſmall to affect an electrometer. In this caſe it is very advantageous to rub thoſe bodies with the metal plate of our apparatus, which plate for this purpoſe muſt be naked; for if the plate be afterwards preſented to an electrometer, this will be electrified conſiderably, however little electricity the rubbed bodies themſelves may have acquired. The quality of this electricity, *viz.* whether it be poſitive or negative, may be eaſily aſcertained, ſince the electricity of the metal plate muſt be the contrary of that acquired by the body rubbed upon it. Mr. CAVALLO alſo made uſe of this method to diſcover the electricity of certain bodies[*]. But there is a better method, to be uſed in caſe the bodies to be examined are not eaſily adapted to the metal plate, which method neither Mr. CAVALLO nor others have known. This is the following. The metal plate being laid upon the imperfectly conducting plane, the body to be tried is rubbed againſt, or is repeatedly ſtroked, upon it; which done, the plate is taken up, and is examined by an electrometer. If the body tried by this method is a piece of leather, a ſtring, a piece of cloth, or velvet, or other imperfect conductor of the like ſort, the plate will be certainly found electrified, and incomparably more by this means than if it were ſtroked by the ſame bodies, whilſt ſtanding inſulated in the air. In ſhort, by either of thoſe methods you will obtain ſome electricity from ſuch bodies as could hardly be expected to give any, even when they are not very dry. Indeed, coals and metals excepted,

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[*] See his Treatiſe on Electricity, part IV. chap. VI.

every other body will give ſome electricity. I can farther ſay, that I have often obtained ſome electricity even by ſtroking the metal plate with my naked hand.

28. It has been queſtioned, whether evaporation, fermentation, &c. produced any electricity, and the inveſtigation is of conſequence for determining ſomething certain about the atmoſpherical electricity. I know that various perſons have attempted in vain to diſcover electricity in thoſe caſes. Some experiments of mine relating to this purpoſe had alſo failed; nevertheleſs, I entertained ſome hopes of ſucceeding, as I had for a great while imagined, that effervеſcence, diſſolution, evaporation, &c. by diſturbing the natural form and ſituation of the particles of bodies, ought to have increaſed or diminiſhed the capacity of the bodies contiguous to thoſe in action, and conſequently ought to have occaſioned in ſome caſes a rarefaction, and in others a condenſation of the electric fluid. Being perſuaded of this theory, I thought that the electricity produced in thoſe caſes was not diſcovered, partly becauſe of its ſmall quantity, and partly becauſe the inſulation was almoſt deſtroyed by the vapours that roſe, and I imagined, that by a greater accuracy, and by multiplying the experiments, I ſhould ſome time or other diſcover it *. It is about two years ſince, that having gradually been able to condenſe the electricity to a great degree by means of the above deſcribed apparatus, I again thought of repeating my old experiments about the evaporation, &c. and entertained much better hopes of diſcovering ſomething new about it, almoſt foreſeeing the event; but various occupations deferred thoſe experiments till the months of March and April of the preſent year 1782, when being at Paris, in company with ſome members of the Royal Academy of Sciences, I at laſt ſucceeded in obtaining clear ſigns of electricity, nay and even the ſpark, from the evaporation of water, from the ſimple combuſtion of coals,

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* All theſe thoughts are mentioned in a Latin diſſertation, printed in the year 1769, and entitled, *De vi attractiva ignis elettrici, ac phænomenis inde pendentibus, ad* JOHANNEM BAPTISTAM BECCARIAM, &c.

and from various effervеſcences, as thoſe which produce inflammable air, fixed air, and nitrous air.

29. I ſhall finiſh the firſt part of this paper with obſerving, that beſides the abovementioned uſes, to which our condenſing apparatus may be applied, the various experiments which may be made with it throw great light upon the theory of electric atmoſpheres in general, of which we are going to treat in the ſecond part.

PART II.

30. The experiments related in the foregoing pages have ſhewn how eaſily a metal plate, or other conducting plain ſurface, when properly ſituated, can draw the electric fluid upon itſelf from a weak atmoſpherical electricity, from a Leyden phial, &c. ſo as to render its effects much more conſpicuous and vigorous. It is now neceſſary to give an explanation of thoſe phenomena, the theory of which will greatly facilitate the practical performance of this ſort of experiments.

31. The whole matter, therefore, may be reduced to this, *viz.* that the metal plate has a much greater capacity for holding electricity in one caſe, *viz.* when it lies upon a proper plane (as mentioned in § 11. 12. 22.) than when it ſtands quite inſulated, as when it is ſuſpended in the air by its ſilk ſtrings or inſulating handle, or when it ſtands upon an inſulating ſtand, as a thick ſtratum of reſin or the like.

32. It is eaſy to comprehend, that wherever the capacity of holding electricity is greater, there the intenſity of electricity

is proportionably leſs, *viz.* a greater quantity of electricity is in that caſe required, in order to raiſe its intenſity to a given degree; ſo that the *capacity* is inverſely as the *intenſity*, by which word I mean the endeavour by which the electricity of an electrified body tends to eſcape from all the parts of it, to which tendency or endeavour the electrical phenomena of attraction, repulſion, and eſpecially the degree of elevation of an electrometer, correſpond.

33. That the *intenſity* of electricity muſt be inverſely proportional to the *capacity* of the body electrified, will be clearly exemplified by the following experiment. Take two metal rods of equal diameter, but one of them a foot, and the other five feet long; and let the firſt be electrified ſo high as that the index of an electrometer annexed to it may be elevated to 60°; then let this electrified rod touch the other rod, and in that caſe it is evident, that the intenſity of the electricity, by being parted between the two rods, will be diminiſhed in proportion as the capacity is increaſed; ſo that the index of the electrometer, which before was elevated to 60°, will now fall to 10°, *viz.* to one-ſixth of the former intenſity, becauſe now the capacity is ſix times greater than when the ſame quantity of electricity was confined to the firſt rod alone. For the ſame reaſon, if the ſaid quantity of electricity was to be communicated to a rod ſixty times longer, its intenſity would be diminiſhed to one degree; and, on the contrary, if the electricity of this long conductor was to be contracted into the ſixtieth part of that capacity, its intenſity would be increaſed to 60°.

34. Now not only conductors of different bulk have different capacities for holding electricity, but alſo the capacity of the ſame conductor may be increaſed or diminiſhed by various circumſtances, ſome of which have not yet been properly conſidered. It has been obſerved, that the capacity of the ſame conductor is increaſed or diminiſhed in proportion as its ſurface is enlarged or contracted, as is ſhewn by Dr. FRANKLIN's experiment of the can and chain, and various other experiments, from which it has been concluded, that the capacity of con-

ductors is in proportion to their ſurface, and not to their quantity of matter.

35. This concluſion is true, but does not comprehend the whole theory, ſince even the extenſion contributes to increaſe the capacity; ſo that of two conductors, which have equal but diſſimilar ſurfaces, that which is the more extended in length has the greater capacity *. In ſhort, it appears from all the experiments hitherto made, that the capacity of conductors is in proportion not to the ſurfaces in general, but *to the ſurfaces which are free*, or *uninfluenced by an homologous atmoſphere.*

36. But that which comes nearer to our caſe is, that the capacity of a conductor, which has neither its form nor ſurface altered, is increaſed when, inſtead of remaining quite inſulated, the conductor is preſented to another conductor not inſulated; and this increaſe is more conſpicuous, according as the ſurfaces of thoſe conductors are larger and come nearer to each other.

When an inſulated conductor is oppoſed or preſented to another conductor whatever, I call it a *conjugate conductor.*

37. The circumſtance mentioned in the preceding paragraph, which augments prodigiouſly the natural capacity of conductors, is that which I find to have been hitherto principally overlooked, far from any advantages having been deduced from it; but let us begin with thoſe experiments which ſhew this increaſed capacity in the ſimpleſt manner. I take, for example, the metal plate of an electrophorus, and holding it by its inſulating handle in the air, electrify it ſo high that the index of an electrometer annexed to it might be elevated to 60°, then lowering this metal plate by degrees towards a table or other conducting plain ſurface, I obſerve that the index of the electrometer falls gradually from 60° to 50°, 40°, 30°, &c. Notwithſtanding this appearance, the quantity of electricity in the plate remains the ſame, except the ſaid plate be brought ſo near the table as to occaſion a tranſmiſſion of the electricity from the

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* See my Diſſertation on the Capacity of Conductors, publiſhed at Milan in the *Opuſcoli Scelti* for the year 1778; and alſo in ROZIER's Journal for the enſuing year.

former to the latter; at leaſt the quantity of electricity will remain as much the ſame as the dampneſs of the air, &c. will permit. The decreaſe, therefore, of intenſity is owing to the increaſed capacity of the plate, which now is not inſulated *ſolitary* but *conjugate*. In proof of this propoſition, if the plate be removed gradually farther and farther from the table, it will be found, that the electrometer riſes again to its former ſtation, namely to 60°, excepting the loſs of that quantity of electricity, which during the experiment muſt have been more or leſs imparted to the air, &c.

38. The reaſon of this phenomenon is eaſily derived from the action of electric atmoſpheres. The atmoſphere of the metal plate, which for the preſent I ſhall ſuppoſe to be electrified poſitively, acts upon the table or other conductor whatever to which it is preſented; ſo that the electric fluid of the table, agreeably to the known laws, retiring to the remoter parts of it, becomes more rare in thoſe parts which are expoſed to the metal plate, and this rarefaction becomes greater the nearer the electrified metal plate is brought to the table. If the metal plate is electrified negatively, then the contrary effects muſt take place. In ſhort, the parts immerſed into the ſphere of action of the electrified metal plate, contract a contrary electricity, which *accidental* electricity, making in ſome manner a compenſation for the *real* electricity of the metal plate, diminiſhes its intenſity, as is ſhewn by the depreſſion of the electrometer (§ 37.).

39. The two following experiments will throw more light upon the reciprocal action of the electric atmoſpheres. Firſt, ſuppoſe two flat conductors, electrified both poſitively or both negatively, to be preſented towards, and to be gradually brought near, each other: it will appear, by two annexed electrometers, that the nearer thoſe two conductors come to each other, the more their intenſities will increaſe; which ſhews, that either of the two *conjugate* conductors has a much leſs capacity now than when it was ſingly inſulated, and out of the influence of the other. This experiment explains the reaſon why an electrified conductor will ſhew a greater intenſity

when it comes to be contracted into a ſmaller bulk; and alſo why a long extended conductor will ſhew a leſs intenſity than a more compact one, ſuppoſing that their quantity of ſurface and of electricity is the ſame; becauſe the homologous atmoſpheres of their parts interfere leſs with each other in the former than in the latter caſe.

40. Secondly, Let the preceding experiment be repeated with this variation only, *viz.* that one of the flat conductors be electrified poſitively, and the other negatively: the effects then will be juſt the reverſe of the preceding, *viz.* the intenſity of their electricities will be diminiſhed, becauſe their capacities are increaſed the nearer the conductors come to each other.

41. Let us now apply the explanation of this laſt experiment to the other experiment mentioned in § 38. *viz.* that of bringing the electrified metal plate towards a conducting plane which is not inſulated; for as this plane acquires a contrary electricity, it follows, that the intenſity of the metal plate's electricity muſt be diminiſhed; hence the annexed electrometer is depreſſed according as the capacity of the plate is increaſed, and conſequently the plate in that caſe may receive a greater quantity of electricity.

42. This matter may be rendered ſtill more clear by inſulating the conducting plane, whilſt the other electrified plate is upon it, and afterwards ſeparating them; for then both the metal plate and the conducting plane (which may be called the *inferior* plane) will be found electrified, but poſſeſſed of contrary electricities, as may be aſcertained by electrometers.

43. If the inferior plane is inſulated firſt, and then the electrified plate is brought over it, then the latter will cauſe an endeavour in the former to acquire a contrary electricity, which, however, the inſulation prevents from taking place; hence the intenſity of the electricity of the plate is not diminiſhed, at leaſt the electrometer will ſhew a very little and almoſt imperceptible depreſſion, which ſmall depreſſion is owing to the imperfection of the inſulation of the inferior plane, and to the ſmall rarefaction and condenſation of the electric fluid, which may take place in different parts of the ſaid inferior

plane. But if in this ſituation the inferior plane be touched ſo as to cut off the inſulation for a moment, then it will immediately acquire the contrary electricity, and the intenſity in the metal plate will be diminiſhed.

44. If the inferior plane, inſtead of being inſulated, were itſelf a non-conducting ſubſtance, then the ſame phenomena would happen, *viz.* the intenſity of the electrified metal plate laid upon it would not be diminiſhed. This, however, is not always the caſe; for if the ſaid inferior non-conducting plane is very thin, and is laid upon a conductor, then the intenſity of the electrified metal plate will be diminiſhed, and its capacity will be increaſed by being laid upon the thin inſulating ſtratum; becauſe in that caſe the conducting ſubſtance, which ſtands under the non-conducting ſtratum, acquiring an electricity contrary to that of the metal plate, will diminiſh its intenſity, &c. and then the inſulating ſtratum will only diminiſh the mutual action of the two atmoſpheres more or leſs, according as it keeps them more or leſs aſunder.

45. The intenſity or electric action of the metal plate, which diminiſhes gradually as it is brought nearer and nearer to a conducting plane not inſulated, becomes almoſt nothing when the plate is nearly in contact with the plane, the compenſation or accidental balance being then almoſt perfect. Hence, if the inferior plane only oppoſes a ſmall reſiſtance to the paſſage of the electricity (whether ſuch reſiſtance is occaſioned by a thin electric ſtratum, or by the plane's imperfect conducting nature, as is the caſe with dry wood, marble, &c.): that reſiſtance joined to the interval, however ſmall, that is between the two planes, cannot be overcome by the weak intenſity of the electricity of the metal plate, which on that account will not dart any ſpark to the inferior plane (except its electricity were very powerful, or its edges not well rounded) and will rather retain its electricity; ſo that, being removed from the inferior plane, its electrometer will nearly recover its former height. Beſides, the electrified plate may even come to touch the imperfectly conducting plane, and may remain in that ſituation for ſome time; in which caſe the intenſity being reduced

almoſt to nothing, the electricity will paſs to the inferior plane exceedingly ſlowly.

46. But the caſe will not be the ſame if, in performing this experiment, the electrified metal plate be made to touch the inferior plane *edgewiſe*; for then its intenſity being greater than when laid flat, as appears by the electrometer, the electricity eaſily overcomes the ſmall reſiſtance, and paſſes to the inferior plane, even acroſs a thin ſtratum *; becauſe the electricity of one plane is balanced by that of the other, only in proportion to the quantity of ſurface which they oppoſe to each other within a given diſtance: whereby when the metal plate touches the other plane in flat and ample contact its electricity is not diſſipated. This apparent paradox is clearly explained by the theory of electric atmoſpheres.

47. What looks more like a paradox is, that neither will the touching the metal plate with a finger, or with a piece of metal, deprive it of all its electricity, whilſt ſtanding upon the proper plane; ſo that it generally leaves it ſo far electrified that, when it is afterwards ſeparated from that plane, it will ſtill afford a ſpark. Indeed this phenomenon could not be explained upon the ſuppoſition that the finger or the metals were perfect conductors. But ſince we do not know of any perfect conductor, the metals or the finger, oppoſe a reſiſtance ſufficient to retard the immediate diſſipation of the electricity of the

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* This explanation, properly applied, renders evident the actions of *points* in general. Properly ſpeaking, a pointed conductor, not inſulated, when preſented to an electrified body, has not in itſelf any particular virtue of attracting electricity. It acts only like a conductor not inſulated, which does not oppoſe any reſiſtance to the paſſage of the electric fluid. If the ſame conductor, inſtead of being pointed, was to preſent a globular or flat ſurface to the electrified body; neither would it in that caſe oppoſe a greater reſiſtance to the paſſage of the electricity. But the reaſon why the electricity will not paſs nearly ſo eaſily from the electrified body to the conductor when it is flat or globular, as when it is pointed, is becauſe in the former caſe the intenſity of the electricity in the electrified body is weakened by the oppoſed flat ſurface, which, acquiring the contrary electricity, compenſates the diminiſhed intenſity incomparably more than a point can. It appears, therefore, that it is not the particular property of a point or of a flat ſurface, but the different ſtate of the electrified body, that makes it part with its electricity eaſier, and from a greater diſtance, when a pointed conducting ſubſtance, than when a flat or globular one is preſented to it.

plate, which is in that caſe actuated by a very ſmall degree of intenſity or endeavour of expanding; ſo that ſuppoſe, for inſtance, that the piece of metal, or the finger by touching the plate, took off ſo much of its electricity as to reduce the intenſity of the remainder to the fiftieth part of a degree; this remaining electricity would then be almoſt nothing; but when the plate, by being ſeparated from the inferior plane, has its capacity ſo far diminiſhed as to render the intenſity of its electricity 100 times greater, then the intenſity of that remaining electricity would become of two degrees or more, *viz.* ſufficient to afford a ſpark.

48. Hitherto we have conſidered in what manner the action of electric atmoſpheres muſt modify the electricity of the metal plate in its various ſituations. We muſt now conſider the effects which take place when the electricity is communicated to the metal plate whilſt ſtanding upon the proper plane. The whole buſineſs having been proved in the preceding pages, it is eaſy to deduce the applications from it; nevertheleſs, it will be uſeful to exemplify it by an experiment. Suppoſe that a Leyden phial or a conductor were ſo weakly electrified that the intenſity of its electricity was only of half a degree or even leſs: if the metal plate of our apparatus, when ſtanding upon the proper plane, was to be touched with that phial or conductor, it is evident, that either of them would impart to it a quantity of its electricity, proportional to the plate's capacity, *viz.* ſo much of it as ſhould make the intenſity of the electricity of the plate equal to that of the electricity in the conductor or phial, ſuppoſe of half a degree; but the plate's capacity, now that it lies upon the proper plane, is above 100 times greater than if it ſtood inſulated in the air, or, which is the ſame thing, it requires 100 times more electricity in order to ſhew the ſame intenſity; therefore, in this caſe it muſt require upwards of 100 times more electricity from the phial or conductor. It naturally follows, that when the metal plate is afterwards removed from the proper plane, its capacity being leſſened ſo as to remain equal to the 100th part of what it was before, the intenſity of its electricity muſt become of 50°; ſince, agree-

ably to the ſuppoſition, the intenſity of the electricity in the phial or conductor was of half a degree.

49. A conductor that is electrified whilſt it ſtands in full and ample contact with another proper conductor, as above ſpecified, and is afterwards ſeparated from it, ſhews the ſame phenomena that are exhibited by a conductor, which, after being electrified, is contracted into a ſmaller bulk, or contrariwiſe, like Dr. FRANKLIN's experiment of the can and chain (§ 35.) &c.

50. If a ſmall quantity of electricity applied to the metal plate of the condenſer enables it to give a ſtrong ſpark, it may be aſked, what would a great quantity of electricity do? The anſwer is, that it would do nothing more, becauſe, when the electricity communicated to the metal plate is ſo ſtrong as to overcome the ſmall reſiſtance of the inferior plane, it will be diſſipated.

51. After all that has been ſaid in the preceding pages, it may be eaſily underſtood, that if the metal plate of our condenſer can receive a good ſhare of electricity from a Leyden phial *, or from an ample conductor, however weakly electrified; it cannot receive any conſiderable quantity of it from a conductor of a ſmall capacity; for this conductor cannot give what it has not, except it were continually receiving a ſtream, howſoever ſmall, of electricity, as is the caſe with an atmoſpherical conductor, or with a prime conductor of an electrical machine, which acts very poorly but continues in action. In thoſe caſes it has been obſerved above (§ 4. 25.) that a conſiderable time is required before the metal plate has acquired a ſufficient quantity of electricity.

52. As an ample conductor, weakly electrified, imparts a conſiderable quantity of electricity to the metal plate of our

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* In my Paper on the Capacity of ſimple Conductors is ſhewn the great capacity of a Leyden phial in compariſon to its bulk, juſt becauſe the electricity, which is communicated to one of its ſurfaces, is balanced by the contrary electricity of the oppoſite ſurface. There I ſhew, that the capacity of 16 ſquare inches of coated ſurface is equal to the capacity of a conductor made of ſilvered cylindrical ſticks, and nearly 100 feet long, the capacity of which is ſo great that its ſpark occaſions a ſhock conſiderably ſtrong.

condenſer, ſo that when the ſaid metal plate is afterwards ſeparated from its proper plane, the electricity in it appears much condenſed and vigorous; ſo when the ſame metal plate contains a ſmall quantity of electricity, and ſuch as cannot give a ſpark or affect an electrometer, that electricity may be rendered very conſpicuous by communicating it to another ſmall metal plate or condenſer.

Mr. CAVALLO was the firſt who thought of this improvement, which he derived by reaſoning upon my experiments. He actually made a ſmall metal plate not exceeding the ſize of a ſhilling: this ſecond condenſer is certainly of great uſe in many caſes, in which the electricity is ſo ſmall as not to be at all, or not clearly, obſervable by my method or a firſt condenſer only, as has been evidently proved by ſome experiments we made together. Sometimes the uſual metal plate of my condenſer acquired ſo ſmall a quantity of electricity, that being afterwards taken up from the inferior plane, and preſented to an extremely ſenſible electrometer of Mr. CAVALLO's conſtruction, it did not affect it. In this caſe, if the ſaid metal plate, thus weakly electrified, was made to touch the other ſmall plate properly ſituated, and that was afterwards brought near an electrometer, the electricity was then generally ſtronger than what would have been ſufficient to aſcertain its quality.

Now, if by the help of both condenſers the intenſity of the electricity has been augmented 1000 times, which is, by no means an exaggeration, how weak muſt then be the electricity of the body examined? how ſmall muſt that electricity be which is produced by rubbing a piece of metal with one's hand, ſince when this electricity is condenſed by both condenſers, and then is communicated to an electrometer, it can hardly affect that inſtrument? Yet it is ſufficient to afford conviction, that the metal can be electrified by the friction of a perſon's hand. Some years ago, *viz.* before the diſcovery of our condenſer, and of Mr. CAVALLO's ſenſible electrometer, we were very far from being able to diſcover ſuch weak excitations; whereas, at preſent, we can obſerve a quantity of electricity incomparably ſmaller than the ſmalleſt obſervable at thoſe times.

APPENDIX.

IN § 28. I mentioned, that after various attempts I at laſt ſucceeded in obtaining undoubted ſigns of electricity from the ſimple evaporation of water, and from various chemical effervescences; but as this is a fact not leſs intereſting than new, it ſeems proper to ſubjoin in this place a faithful account of the experiments made for that purpoſe. The firſt ſet of experiments were made at Paris, in company with Mr. LAVOISIER and Mr. DE LA PLACE, two intelligent philoſophers and members of the Royal Academy of Sciences. After I had ſhewn them my experiments with my condenſer, they, as well as myſelf, began to entertain hopes of ſucceeding in the experiments on the evaporation, &c. Accordingly Mr. LAVOISIER ordered a large condenſer with a marble plane to be made. The firſt experiment I attempted with this inſtrument, in company with Mr DE LA PLACE, proved unſucceſsful; but the weather at that time was bad, the room was narrow and full of vapours, and the apparatus was not quite in proper order. Mr. DE LA PLACE and Mr. LAVOISIER repeated thoſe experiments in the country, and then they were attended with ſucceſs, which incited us to repeat and diverſify the experiments, by which means the diſcovery was compleated; having obtained unequivocal ſigns of electricity from the evaporation of water, from the ſimple combuſtion of coals, and from the effervescence of iron filings in diluted vitriolic acid. This obſervation was made the 13th of April of the preſent year 1782, and the experiments were performed in the following manner. In an open garden a long metal plate was inſulated, which, by means of a large iron wire, was made to communicate with the metal plate of the condenſer laid upon the piece of marble, which was kept continually warm by ſome lighted coals ſet underneath. This done, ſome chafing-diſhes, containing burning charcoal, were placed upon the large inſulated plate. The combuſtion of the coals was helped by a gentle wind. Some minutes after, the iron wire, by which the large inſulated plate was connected with the metal plate of the condenſer, was taken off; then the metal plate being removed from the marble by its inſulated handle, and preſented to Mr. CAVALLO's electrometer, made the balls of it diverge with negative electricity. The experiment was repeated by placing upon the large inſulated plate four veſſels, containing iron filings and water, inſtead of the chafing-diſhes: then ſome vitriolic acid was poured into thoſe four veſſels, ſufficient to cauſe a vigorous effervescence, and when the ſtrongeſt ebullition was going to ſubſide, the metal plate of the condenſer was removed from over the marble; and being examined, not only electrified the electrometer with negative electricity, but gave a ſenſible ſpark. At this time having tried to obtain electricity from the evaporation of water, the effects were equivocal or hardly ſenſible; the ſame thing happened a few days after, when however we obtained clear ſigns of electricity from thoſe effervescences, which produce fixed and nitrous air. Thoſe experiments were made in a large room.

One day the electricity ariſing from the evaporation of water ſeemed to be poſitive; but ſubſequent experiments, and other circumſtances, indicate that ſuch a phenomenon muſt be attributed to a miſtake.

Once on repeating theſe experiments in company with Mr. LE ROY, member of the R. A. of Sciences, we could not obtain any electricity from the evaporation of water or from combuſtion, the weather being extremely damp; but the effervescence of iron filings and diluted vitriolic acid produced electricity enough to aſcertain that it was negative, though it afforded no ſpark.

A ſhort time before I left Paris I once more repeated the experiment of the effervescence of iron filings, &c. with ſucceſs. This experiment was made in the laboratory of Mr. BILLAUM, an inſtrument-maker and lover of electricity.

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The experiment on the evaporation of water, which did not anſwer ſo well at Paris, ſucceeded much better in London, where I bethought me of throwing water upon the lighted coals, which were kept in an inſulated chafing-diſh. In this manner the electricity of the evaporation never fails to electrify the chafing-diſh negatively, and ſtrongly enough for the electricity to be diſcovered by the ſimple electrometer; it will even afford a ſpark, if the condenſer is uſed. The firſt experiment of this ſort was made at Mr. BENNET's, who is a great lover of electricity, in preſence of Mr. BENNET, Mr. CAVALLO, and Mr. KIRWAN, members of the Royal Society, and of Mr. WALKER, lecturer of experimental philoſophy.

Another time this experiment was repeated with ſucceſs at Mr. CAVALLO's, in the following manner. A ſmall crucible, containing three or four ſmall coals lighted, was inſulated; then a ſpoonful of water was thrown upon the coals, and immediately after an electrometer, which communicated with the coals by means of a wire, diverged with negative electricity.

Theſe are the experiments which I have had the opportunity to make hitherto; in relating which I muſt not omit to obſerve, that although the condenſing apparatus has not been always indiſpenſably neceſſary, Mr. CAVALLO's very ſenſible electrometer alone having been often ſufficient for the purpoſe; yet it muſt be confeſſed, that it was the condenſing apparatus which ſuggeſted theſe experiments, and by the help of which even the electric ſpark could be obtained. Theſe experiments have juſt opened the way to a vaſt field, which deſerves much farther inveſtigation. It is natural to ſuppoſe, that if compact bodies, when they are rarefied or become an elaſtic fluid, require an additional quantity of electric fluid, and conſequently leave thoſe bodies, with which they are connected, negatively electrified; it muſt happen, on the contrary, that when vapours condenſe they muſt part with ſome electric fluid, that is, muſt produce a poſitive electricity. This, however, remains to be proved experimentally, and I have already imagined ſeveral ways of trying it, which will be put in practice as ſoon as I ſhall have the opportunity. Mean while I beg leave to conclude this paper with mentioning a few ideas I entertain relating to the atmoſpherical electricity.

The experiments hitherto made, though not numerous, yet concur to ſhew, that the vapours of water, and in general the parts of all bodies, that are ſeparated by volatilization, carry away an additional quantity of electric fluid as well as of elementary heat, and conſequently that thoſe bodies, from the contact of which the volatile particles have been ſeparated, remain both cooled and electrified negatively: from which it may be deduced, that whenever bodies are reſolved into volatile elaſtic fluid, their capacity for holding electric fluid is augmented, as well as their capacity for holding common fire, or the calorific fluid. This is a ſtriking analogy by which the ſcience of electricity throws ſome light upon the theory of heat, and alternately derives light from it; I mean on the doctrine of latent or ſpecific heat, the firſt notions of which were ſuggeſted by the admirable experiments of Dr. BLACK and WILKE, and which has been afterwards much elucidated by Dr. CRAWFORD, who followed the experiments of Dr. IRWIN.

By following this analogy it ſeems, that as the vapours on their condenſing, loſe part of their latent heat, on account of their capacity being diminiſhed, ſo they part with ſome electric fluid. Hence originates the poſitive electricity, which is always more or leſs predominant in the atmoſphere, when the ſky is clear, *viz.* at that height where the vapours begin to be condenſed. Accordingly, the atmoſpherical electricity is ſtronger in fogs, in which caſe the vapours are more condenſed, ſo as to be almoſt reduced into drops, and is ſtill ſtronger when thick fogs become clouds.

Hitherto we have accounted for the poſitive atmoſpherical electricity; but it is eaſy to account for clouds negatively electrified; for when a cloud, poſitively electrified, has been once formed, its ſphere of action is extended a great way round, ſo that if another cloud comes within that ſphere, its electric fluid, agreeably to the well known laws of electric atmoſpheres, muſt retire to the parts of it which are the remoteſt from the firſt cloud; and from thence the electric fluid may be communicated to other clouds, or vapours, or terreſtrial prominencies. Thus a cloud may be electrified negatively, which cloud, after the ſame manner, may occaſion a poſitive electricity in another cloud, &c. This explains not only the negative electricity, which is often obtained from the atmoſphere in cloudy weather; and the frequent changes from poſitive to negative electricity, and contrariwiſe in ſtormy weather; but alſo the waving motion often obſerved in the clouds, and the hanging down of them, ſo as nearly to touch the earth.

After the fore-mentioned diſcoveries we need no longer wonder at the appearance of lightnings in the eruptions of volcanos, as was particularly obſerved in the late dreadful eruption of Mount Veſuvius. The few experiments I have made ſhew, that the quantity of ſmoke, but much more the rapidity with which it is produced, tends to increaſe the electricity which ariſes from combuſtion, &c. How great muſt then be the quantity of electricity that is produced in ſuch eruptions?

AMENDMENT to P. 191. L. 12.

I have lately repeated this experiment, and found that one meaſure of alkaline air is ſaturated by leſs than half of one meaſure of fixed air, but more than one-third, conformably to Dr. PRIESTLEY's firſt experiment, p. 293.; by which it appears, that 100 gr. of alkaline air require about 120 of fixed air to ſaturate them: and hence 100 gr. of concrete volatile alkali contain about 53 of fixed air; 44 of mere volatile alkali, and 3 of water.

E R R A T A.

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Page.	Line.	
9.	20.	*for* 0,03 *read* 0,035
11.	26.	*for* 3,100 *read* 3106.
18.	12.	*for* $\frac{3,65}{0,290}$ *read* $\frac{3,55}{0,299}$

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73.	12.	*for* one fortieth *read* one twentieth
75.	1.	*for* five ſevenths *read* five fourths
78.	9.	*for* ſevenths of an inch *read* fourths of an inch
81.	2.	*for* now *read* more
117.	11.	*for* 38 *read* 39
127.	8.	*for* 108 *read* 107
136.	24.	*for* ζ Capricorni *read* FL. 64. Sagittarii
147.	3.	*for* the 1½ *read* the fartheſt 1½
149.	28.	*for* 40′ *read* 40″
153.	7.	*for* π FL. 10. *read* ρ FL. 11.
155.	4.	*for* 5 *read* 4
161.	21.	*for* 6″ 27′ *read* 6″ 27‴
182.		*at the top of the 6th column, inſert* Phyſical ſpecific gravity.
——		*firſt ſeries of the 6th column, for* 1,846 *read* 1,8846
183.		*at the top of the 6th column, inſert* Phyſical ſpecific gravity.
185.	12.	*for* 1,4653 *read* 1,4650
206.	14.	*after* nitrous *read* teſt
217.	23. and 26.	*for* 1 PR. *read* 4 PR.

PHILOSOPHICAL TRANSACTIONS,

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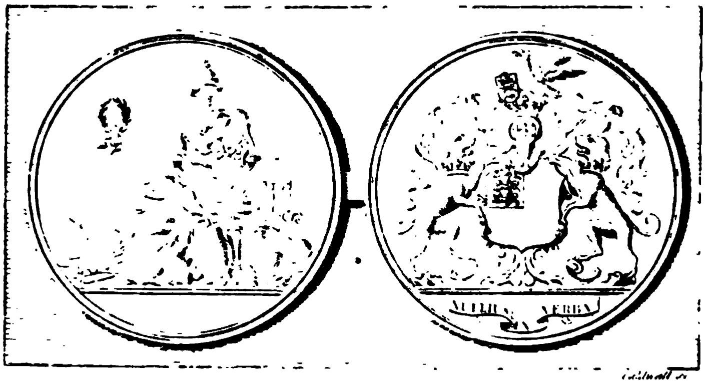

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PHILOSOPHICAL

TRANSACTIONS.

XIX. *An Attempt to make a Thermometer for meaſuring the higher Degrees of Heat, from a red Heat up to the ſtrongeſt that Veſſels made of Clay can ſupport. By* Joſiah Wedgwood; *communicated by Sir* Joſeph Banks, *Bart. P. R. S.*

Read May 9, 1782.

A MEASURE for the higher degrees of heat, ſuch as the common thermometers afford for the lower ones, would be an important acquiſition, both to the philoſopher and the practical artiſt. The latter muſt feel the want of ſuch a meaſure on many occaſions; particularly when he attempts to follow, or apply to uſe, the curious experiments of Mr. POTT, related in his Lithogeognoſia, and other modern writers upon ſimilar ſubjects. When we are told, for inſtance, that ſuch and ſuch materials were changed by fire into a fine white, yellow, green, or other coloured glaſs: and find, that theſe effects do not happen, unleſs a particular degree of fire has fortunately been hit upon, which degree we cannot be ſure of ſucceeding in again:—when we are diſappointed, by having the reſult at ſome times an unvitrified maſs, and at others an over-vitrified ſcoria, from a little deficiency or exceſs of heat:—when we ſee colours altered, not only in ſhade but in kind, and in many caſes deſtroyed, by a ſmall augmentation of the heat which had produced them; inſomuch, that in the gradual increaſe of the fire, a preciſe moment of time muſt be happily ſeized, in order to catch them in perfection:—and when inconveniences, ſimilar to theſe, ariſe in operations by fire upon metals and other ſubſtances:—how much is it to be wiſhed, that the authors had been able to convey to us a meaſure of the heat made uſe of in their valuable proceſſes!

In a long courſe of experiments, for the improvement of the manufacture I am engaged in, ſome of my greateſt difficulties and perplexities have ariſen from not being able to aſcertain the heat to which the experiment-pieces had been expoſed. A *red*, *bright red*, and *white heat*, are indeterminate expreſſions; and even though the three ſtages were ſufficiently diſtinct from each other, they are of too great latitude; as the brightneſs or luminouſneſs of fire increaſes, with its force, through numerous gradations, which can neither be expreſſed in words, nor diſcriminated by the eye. Having no other reſource, I have been obliged to content myſelf with ſuch meaſures as my own kilns and the different parts of them afforded. Thus the kiln in which our glazed ware is fired furniſhes three meaſures, the bottom being of one heat, the middle of a greater, and the top ſtill greater: the kiln in which the biſcuit ware is fired furniſhes three or four others, of higher degrees of heat; and by theſe I have marked my regiſtered experiments. But though theſe meaſures had been fully adequate to my own views, which they were not, it is plain, that they could not be communicated to others; that their uſe is confined to a particular ſtructure of furnaces, and mode of firing; and that, upon any alteration in theſe, they would become uſeleſs and unintelligible, even where now they are beſt known. And, indeed, as this part of the operation is performed by workmen of the loweſt claſs, we cannot depend upon any great accuracy even in one and the ſame furnace. It has accordingly often happened, that the pieces fired in the top of the kiln in one experiment have been made no hotter than thoſe fired in the middle in another, and *vice verſâ*.

The force of fire, in its higher as well as lower ſtages, can no otherwiſe be juſtly aſcertained than by its effects upon ſome known body. Its effect in changing colours has already been hinted at; and I have obſerved compoſitions of calces of iron with clay to aſſume, from different degrees of fire, ſuch a number of diſtinct colours and ſhades as promiſed to afford uſeful criteria of the reſpective degrees.

With this idea, I prepared a quantity of ſuch a compoſition, and formed it into circular pieces, about an inch in diameter, and a quarter of an inch thick. A number of theſe was placed in a kiln, in which the fire was gradually augmented, with as much uniformity and regularity as poſſible, for near ſixty hours. The pieces, taken out at equal intervals of time during this ſucceſſive increaſe of heat, and piled in their order upon one another in a glaſs tube, exhibited a regular and pretty extenſive ſeries of colours; from a fleſh-colour to a deep browniſh-red, from thence to a chocolate, and ſo on to nearly black, with all the intermediate tints between theſe colours. A back being fixed to the tube, like the ſcale of a thermometer, and the numbers of the pieces marked upon it reſpectively oppoſite to them, it is obvious, that theſe numbers may be conſidered as ſo many thermometric diviſions or degrees; and that, if another piece of the ſame compoſition be fired in any other kiln or furnace, not exceeding the utmoſt heat of the firſt, it will acquire a colour correſponding to ſome of the pieces in the tube, and thus point out the degree of heat which that piece, and conſequently ſuch other matters as were in the fire along with it, have undergone.

It muſt however be confeſſed, that, for general uſe, a thermometer on this principle is liable to objection, as ideas of colours are not perfectly communicable by words; nor are all eyes, or all lights, equally adapted for diſtinguiſhing them, eſpecially the ſhades which approach near to one another; and the effects of phlogiſtic vapours, in altering the colour, may not in all caſes be eaſily guarded againſt.

In conſidering this ſubject attentively, another property of argillaceous bodies occurred to me; a property which obtains, in a greater or leſs degree, in every kind of them that has come under my examination, ſo that it may be deemed a diſtinguiſhing character of this order of earths: I mean, the *diminution of their bulk by fire*; I have the ſatisfaction to find, in a courſe of experiments lately made with this view, that it is a more accurate and extenſive meaſure of heat than the different ſhades of colour.

I have found, that this diminution begins to take place in a low red-heat; and that it proceeds regularly, as the heat increaſes, till the clay becomes vitrified, and conſequently to the utmoſt degree that crucibles, or other veſſels made of this material, can ſupport. The total contraction of ſome good clays which I have examined, in the ſtrongeſt of my own fires, is conſiderably more than one-fourth part in every dimenſion.

If, therefore, we can procure at all times a clay ſufficiently apyrous or unvitreſcible, and always of the ſame quality in regard to contraction by heat; and if we can find means of meaſuring this contraction with eaſe and minute accuracy, I flatter myſelf, that we ſhall be furniſhed with a meaſure of fire ſufficient for every purpoſe of experiment or buſineſs.

We have, in different parts of England, immenſe beds of clay; each of which, at equal depths, is pretty uniform in quality throughout its whole extent. Of all the ſorts I have hitherto tried, ſome of the pureſt Corniſh porcelain clays ſeem the beſt adapted, both for ſupporting the intenſity, and meaſuring the degrees, of fire.

For preparing and applying this material to thermometric purpoſes the following method is propoſed.

The clay is firſt to be waſhed over, and, whilſt in a dilute ſtate, paſſed through a fine lawn. Let it then be made dry, and put up in boxes *,

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* While the clay is thus kept dry in boxes, as well as while it continues in its natural bed, it is ſecure from alterations in quality, which clays in general are ſubject to undergo, when expoſed, for a long courſe of years, to the joint actions of air and moiſture.—In the lawns I made uſe of, the interſtices were each leſs than the 100,000 part of an inch.

The dry clay is to be ſoftened, for uſe, with about two-fifths of its weight of water; and formed into ſmall pieces, in little moulds of metal, ſix-tenths of an inch in breadth, with the ſides pretty exactly parallel, this being the dimenſion intended to be meaſured, about four-tenths of an inch deep, and one inch long. To make the clay deliver eaſily, it will be neceſſary to oil the mould, and make it warm.

Theſe pieces, when perfectly dry, are put into another iron mould or gage, conſiſting only of a bottom, with two ſides, five-tenths of an inch deep; to the dimenſions of which ſides the breadth of the pieces is to be pared down.

For meaſuring the diminution which they are to ſuffer from the action of fire, another gage is made, of two pieces of braſs, twenty-four inches long, with the ſides exactly ſtraight, divided into inches and tenths, fixed five tenths of an inch aſunder at one end, and three-tenths at the other, upon a braſs plate; ſo that one of the thermometric pieces, when pared down in the iron gage, will juſt fit to the wider end. Let us ſuppoſe this piece to have diminiſhed in the fire one-fifth of its bulk, it will then paſs on to half the length of the gage; if diminiſhed two-fifths, it will go on to the narroweſt end; and in any intermediate degree of contraction, if the piece be ſlid along till it reſts againſt the converging ſides, the degree at which it ſtops will be the meaſure of its contraction, and conſequently of the degree of heat it has undergone.

Theſe are the outlines of what appears to me neceſſary for the making and uſing of this thermometer; and it is hoped, that the whole proceſs will be found ſufficiently ſimple, and eaſy of execution. It may, nevertheleſs, be proper to take notice of a few minuter circumſtances, and to mention ſome obſervations which occurred in the progreſs of the inquiry.

I. There ought to be a certainty of the clay being eaſily, and at all times, procurable in ſufficient quantity, and on moderate terms. That this is the caſe with the clay here made choice of, will be evident to every one acquainted with the natural hiſtory of Cornwall, where there are beds of this clay, inexhauſtible, and in too many hands to be monopolized. If this ſhould not prove ſatisfactory, the author offers to this illuſtrious Society, and will think himſelf honoured by their acceptance of, a ſufficient ſpace in a bed of this clay to ſupply the world with thermometer-pieces for numerous ages; and he does not apprehend, that any greater inconveniences can ariſe to foreign artiſts or philoſophers, from their being ſupplied with clay for theſe thermometers from this ſpot only, than what we now feel from being ſupplied with mercury for the common thermometers from the Spaniſh or Hungarian mines.

II. We ought to be aſſured alſo, that all the clay made uſe of for theſe thermometers is perfectly ſimilar. For this purpoſe, it will be beſt to dig it out of the earth in conſiderable quantity at once, an extent of ſome ſquare feet or yards in area, and to the depth of ſix or ſeven yards or more from the ſurface, and to mix the whole thoroughly together, previous to the further preparation already mentioned. When the firſt quantity is exhauſted, another perpendicular column may be dug from the ſame bed, cloſe to the firſt, to the ſame depth, and prepared in the ſame manner; by which means we may be aſſured of its ſimilarity with the former parcel, and that it will diminiſh equally in the fire.

III. This clay, dried by the ſummer heat, or in a moderately warm room, or with more heat before a fire, has not been obſerved to differ in *degree* of dryneſs. After being ſo dried, it loſes about a hundredth part of its weight in the heat of boiling water, about as much more in that of melted lead, and from thence to a red-heat ten parts, in all $\frac{12}{100}$. Each of theſe heats ſoon expels from the clay its determinate quantity of matter, chiefly air; after which, the ſame heat, though continued for many hours, has no further effect. I had ſome hopes, that the graduation of the common thermometer might be continued, upon this principle, up to the red-heat at which the ſhrinking of the clay commences, ſo as to connect the two thermometers together by one ſeries of numbers; but the loſs of weight appears not to be ſufficiently uniform or proportional to the degree of heat to anſwer that purpoſe; for it was found to go on quicker, and bladders tied to the mouths of the veſſels in which the pieces were heated, became more rapidly diſtended, at the commencement of redneſs than at any other time. From low red-heat to a ſtrong one, ſuch as copper melts in, the loſs of weight was only about two parts in a hundred; though the difference between theſe two heats appears to be much greater than what the ſame loſs correſponds to in the lower ſtages. After this period, the decreaſe of weight intirely ceaſed.

The vapours expelled from the clay, caught ſeparately in the different degrees of heat, ſeemed, from the few trials made with them, to conſiſt of common air mixed with fixt air. They all precipitated lime-water; that which was firſt extricated, exceeding weakly; the others more and more conſiderably; but the laſt not near ſo ſtrongly as the air expelled from lime-ſtone in burning. None of them were inflammable.

IV. The thermometric pieces may be formed much more expeditiouſly than in the ſingle mould, by means of an inſtrument uſed for ſimilar purpoſes by the potters. It conſiſts of a cylindrical iron veſſel, with holes, in the bottom, of the form and dimenſions required. The ſoft clay, put in the veſſel, is forced by a preſs down through theſe apertures, in long rods, which may be cut while moiſt, or broken when dry, into pieces of convenient lengths. It was hoped, that this method would of itſelf have been ſufficient, without the addition of the paring gage, making proper allowance, in the ſize of the holes, for the ſhrinking of the clay in drying. But it was found, that a variety of little accidents might happen to alter the ſhape and dimenſions of the pieces, in a ſenſible degree, while in their ſoft ſtate; ſo that it will be always ſafeſt to have recourſe to the paring gage, for aſcertaining and adjuſting their breadth when perfectly dry, this being the period at which the pieces are exactly alike with regard to their future diminiſhing; ſo that if they are now reduced to the ſame breadth, we may be ſure that they will ſuffer equal contractions from equal degrees of heat afterwards, whether they have been made in a mould, or by a preſs, or in any other way; neither is any variation in the length or thickneſs of theſe pieces of the leaſt conſequence, provided one of the dimenſions, that by which they are afterwards to be meaſured, is made accurate to the gage.

V. It will be proper to bake the pieces, when dry, with a low red-heat, in order to give them ſome firmneſs or hardneſs, that they may, if neceſſary, be able to bear package and carriage; but more eſpecially to prepare them for being put into an immediate heat, along with the matters they are to ſerve as meaſures to, without burſting or flying, as unburnt clay would do. We need not be ſolicitous about the preciſe degree of heat employed in this baking, provided only that it does not exceed the loweſt degree which we ſhall want to meaſure in practice; for a piece that has ſuffered any inferior degrees of heat, anſwers as well for meaſuring higher ones as a piece which has never been expoſed to fire at all. In this part of the preparation of the pieces, it may be proper to inform the operator of a circumſtance, which, though otherwiſe immaterial, might at firſt diſconcert him: if the heat is not in all of them exactly equal, he will probably find, that while ſome have begun to ſhrink, others are rather enlarged in their bulk; for they all ſwell a little juſt on the approach of redneſs. As this is the period of the moſt rapid produce of air, the extenſion may perhaps be owing to the air having at this moment become elaſtic to ſuch a degree, as to force the particles of the clay a little aſunder before it obtains its own enlargement.

VI. Each diviſion of the ſcale, though ſo large as a tenth of an inch, anſwers to $\frac{1}{600}$th part of the breadth of the little piece of clay. We might go to much greater nicety, either by making the diviſions ſmaller, or the ſcale longer; but it is not apprehended, that any thing of this kind will be found neceſſary: and, indeed, in proceeding much further in either way, we may poſſibly meet with inconveniences ſufficient to counterbalance the apparent additional accuracy of meaſurement.

VII. The diviſions of this ſcale, like thoſe of the common thermometers, are unavoidably arbitrary; but the method here propoſed appears ſufficiently commodious and eaſy of execution, the diviſions being adjuſted by meaſures everywhere known, and at all times obtainable: for however the inches uſed in different countries may differ in length, this cannot affect the accuracy of the ſcale, provided that the proportions between the wider and narrower end of the gage are exactly as five-tenths of thoſe inches to three-tenths, and the length 240 of the ſame tenths; and that the pieces in their perfectly dry ſtate, before firing, fit preciſely to the wider end. When one gage is accurately adjuſted to theſe proportional meaſures, two pieces of braſs ſhould be made, one fitting exactly into one end, and the other into the other; theſe will ſerve as ſtandards for the ready adjuſtment of other gages to the dimenſions of the original.

By this ſimple method we may be aſſured, that thermometers on this principle, though made by different perſons, and in different countries, will all be equally affected by equal degrees of heat, and all ſpeak the ſame language: the utility of this laſt circumſtance is now too well known to need being inſiſted on.

VIII. If a ſcale two feet in length ſhould be reckoned inconvenient, it may be divided into two, of one foot each, by having three pieces of braſs fixed upon the ſame plate; the firſt and ſecond, five-tenths of an inch apart at one end, and four-tenths at the other; the ſecond and third, four-tenths at one end, and three-tenths at the other; ſo that the firſt reaches to the 120th diviſion, and the ſecond from thence to the 240th.

IX. As this thermometer, like all others, can expreſs only the heat felt by itſelf, the operator muſt be careful to expoſe the pieces to an equal action of the fire with the body whoſe heat he wants to meaſure by them. In kilns, ovens, reverberatories, under a muffle, and wherever the heat is pretty ſteady and uniform, the means of doing this are too obvious to need being mentioned. But in a naked fire, where the heat is neceſſarily more fluctuating, and unequal in different parts of the fuel, ſome precaution will be required.

The thermometer-piece may generally be put into the crucible, along with the ſubject-matter of the experiment. But where the matter is of ſuch a kind as to melt and ſtick to it, the piece may be previouſly incloſed in a little caſe made of crucible clay. The ſmallneſs of the pieces will admit of this being done without inconvenience, at leaſt in any but the ſmalleſt crucibles, as the pieces themſelves may be diminiſhed to any ſize that may be found proper, provided only that one of the dimenſions, five-tenths of an inch, be preſerved, as mentioned in *Obſ.* 4.

For the very ſmalleſt ſort of crucibles, the caſe may be put in cloſe to the crucible, ſo as to form as it were an addition to its bulk on the outſide. If it be aſked, why the caſe is not always thus put in by the ſide of the crucible? it is anſwered, that in judging of the heat of *large* crucibles from a thermometer-piece placed on the outſide of them, we may ſometimes be deceived, as the piece in its little caſe has been found to heat ſooner than the matter in the larger veſſel; but in *ſmall* ones, as the crucible and caſe are nearly alike in bulk, there is little danger of error from this cauſe.

X. Theſe thermometer-pieces poſſeſs ſome ſingular properties, which we could not have expected to find united in any ſubſtance whatever, and which peculiarly fit them for the purpoſes they are here applied to.

1. When baked by only moderate degrees of fire, though they are, like other clays, of a porous texture, and imbibe

water;

water; yet, when ſaturated with the water, their bulk continues exactly the ſame as in a dry ſtate.

2. By very ſtrong fire, they are changed to a porcelain or ſemi-vitreous texture; nevertheleſs, their contraction, on further augmentations of the heat, proceeds regularly as before, up to the higheſt degree of fire that I have been able to produce.

3. They bear ſudden alternatives of heat and cold; may be dropped at once into intenſe fire, and, when they have received its heat, may be plunged as ſuddenly into cold water, without the leaſt injury from either.

4. Even while ſaturated with water in their porous ſtate, they may be thrown immediately into a white heat, without burſting or ſuffering any injury.

5. Sudden cooling, which alters both the bulk and texture of moſt bodies, does not at all affect theſe, at leaſt not in any quality ſubſervient to their thermometric uſes.

6. Nor are they affected by long *continuance* in, but ſolely by, the *degree* of heat they are expoſed to. In three minutes or leſs, they are perfectly penetrated by the heat which acts upon them, ſo as to receive the full contraction which that degree of heat is capable of producing equally with thoſe which had undergone its action during a gradual increaſe of its force for many hours. Strong degrees of heat are communicated to them with more celerity than weak ones: perhaps the heat may be more readily tranſmitted, in proportion as the texture becomes more compact.

Theſe facts have been aſcertained by many experiments, the particulars of which are omitted, becauſe they would ſwell this paper much beyond the bulk intended.

XI. The uſe and accuracy of this thermometer for meaſuring, *after an operation*, the degree of heat which the matter has undergone, will be apparent. The foregoing properties afford means of meaſuring it alſo, eaſily and expeditiouſly, *during the operation*, ſo that we may know when the fire is increaſed to any degree previouſly determined upon. The piece may be taken out of the fire in any period of the proceſs, and dropped immediately into water, ſo as to be fit for meaſuring by the gage in a few ſeconds of time. At the ſame inſtant, another piece may be introduced into the place of the former, to be taken out and meaſured in its turn; and thus alternately, till the deſired degree of heat is obtained. But as the cold piece will be two or three minutes in receiving the full heat, and correſponding contraction; to avoid this loſs of time, it may be proper, on ſome occaſions, to have two or more pieces, according to convenience, put in together at firſt, that they may be ſucceſſively cooled in water, and the degrees of heat examined at ſhorter intervals. It will be unneceſſary to ſay any thing further upon precautions or procedures which the very idea of a thermometer muſt ſuggeſt, and in which it is not apprehended that any difficulty can occur, which every experimenter will not readily find means to obviate.

XII. It now only remains, that the language of this new thermometer be underſtood, and that it may be known what the heats meant by its degrees really are. For this purpoſe a great number of experiments has been made, from which the following reſults are ſelected.

The ſcale commences at a red-heat, fully viſible in day-light; and the greateſt heat that I have hitherto obtained in my experiments is 160°. This degree I have produced in an air-furnace about eight inches ſquare.

Mr. ALCHORNE has been ſo obliging as to try the neceſſary experiments with the pure metals at the Tower, to aſcertain at what degrees of this thermometer they go into fuſion; and it appears, that Swediſh copper melts at 27, ſilver at 28, and gold at 32.

Braſs is in fuſion at 21. Nevertheleſs, in the braſs and copper founderies, the workmen carry their fires to 140° and upwards: for what purpoſe they ſo far exceed the melting heat, or whether ſo great an additional heat be really neceſſary, I have not learnt.

The welding heat of iron is from 90 to 95; and the greateſt heat that could be produced in a common ſmith's forge 125.

Caſt iron was found to melt at 130°, both in a crucible in my own furnace, and at the foundery; but could not be brought into fuſion in the ſmith's forge, though that heat is only 5° lower. The heat by which iron is run down among the fuel for caſting is 150°.

As the welding ſtate of iron is a ſoftening or beginning fuſion of the ſurface, it has been generally thought that caſt iron would melt with much leſs heat than what is neceſſary for producing this effect upon the forged; whereas, on the contrary, caſt iron appears to require, for its fuſion, a heat exceeding the welding heat 35 or 40°, which is much more than the heat of melted copper exceeds the loweſt viſible redneſs.

Thus we find, that though the heat for melting copper is by ſome called a white heat, it is only 27° of this thermometer. The welding heat of iron, or 90°, is likewiſe a white heat; even 130°, at which caſt iron is in fuſion, is no more than a white heat; and ſo on to 160° and upwards is all a white heat ſtill. This ſhews abundantly how vague ſuch a denomination muſt be, and how inadequate to the purpoſe of giving us any clear ideas.

ideas of the extent of what we have been accuſtomed to conſider as one of the three diviſions of heat in ignited bodies.

A Heſſian crucible, in the iron foundery, *viz.* about 150°, melted into a ſlag-like ſubſtance. Soft iron nails, in a Heſſian crucible in my own furnace, melted into one maſs with the bottom of the crucible, at 154°: the part of the crucible above the iron was little injured.

The *fonding* heat of the glaſs furnaces I examined, or that by which the perfect vitrification of the materials is produced, was at one of them 114° for flint-glaſs, and 124° for plate-glaſs; at another it was only 70° for the former, which ſhews the inequality of heat, perhaps unknown to the workmen themſelves, made uſe of for the ſame purpoſe. After complete vitrification, the heat is abated for ſome hours to 28 or 29°, which is called the *ſettling* heat; and this heat is ſufficient for keeping the glaſs in fuſion. The fire is afterwards increaſed, for working the glaſs, to what is called the *working* heat; and this I found, in plate-glaſs, to be 57°.

Delft ware is fired by a heat of 40 or 41°; cream-coloured, or Queen's ware, by 86°; and ſtone ware, called by the French *pots de grès*, by 102°: by this ſtrong heat, it is changed to a true porcelain texture. The thermometer-pieces begin to acquire a porcelain texture about 110°.

The above degrees of heat were aſcertained by thermometer-pieces fired along with the ware in the reſpective kilns. But this thermometer affords means of doing much more, and going further in theſe meaſures than I could at firſt even have expected; it will enable us to aſcertain the heats by which many of the porcelains and earthen wares of diſtant nations and different ages have been fired: for as burnt clay, and compoſitions in which clay is a prevailing ingredient, ſuffer no diminution of their bulk by being re-paſſed through degrees of heat which they have already undergone, but are diminiſhed by any additional heat (according to *Obſ.* V.), if a fragment of them be made to fit into any part of the gage, and then fired along with a thermometer-piece till it begins to diminiſh, the degree at which this happens points out the heat by which it had been fired before. Of ſeveral pieces of ancient Roman and Etruſcan wares, which I have examined, none appear to have undergone a greater heat than 32°, and none leſs than 20°; for they all began to diminiſh at thoſe or the intermediate degrees.

By means of this thermometer ſome intereſting properties of natural bodies may likewiſe be diſcovered, or more accurately determined, and the genus of the bodies aſcertained. Jaſper, for inſtance, is found to diminiſh in the fire, like an artificial mixture of clay and ſiliceous matter; granite, on the contrary, has its bulk enlarged by fire, whilſt flint and quartzoſe ſtones are neither enlarged nor diminiſhed. Theſe experiments were made in fires between 70 and 80° of this thermometer. A ſufficient number of facts like theſe, compared with each other, and with the properties of ſuch natural or artificial bodies as we wiſh to find out the compoſition of, may lead to various diſcoveries, of which I have already found ſome promiſing appearances; but many more experiments are wanting to enable me to ſpeak with that certainty and preciſion on theſe ſubjects which they appear to deſerve.

A piece of an Etruſcan vaſe melted completely at 33°; pieces of ſome other vaſes and Roman ware about 36°; Worceſter china vitrified at 94°; Mr. SPRIMONT's Chelſea china at 105°; the Derby at 112°; and Bow at 121°; but Briſtol china ſhewed no appearance of vitrification at 135°. The common ſort of of Chineſe porcelain does not perfectly vitrify by any fire I could produce; but began to ſoften about 120°, and at 156° became ſo ſoft as to ſink down, and apply itſelf cloſe upon a very irregular ſurface underneath. The true ſtone Nankeen, by this ſtrong heat, does not ſoften in the leaſt; nor does it even acquire a porcelain texture, the unglazed parts continuing in ſuch a ſtate as to imbibe water and ſtick to the tongue. The Dreſden porcelain is more refractory than the common Chineſe, but not equally ſo with the ſtone Nankeen. The cream-coloured or Queen's ware bears the ſame heat as the Dreſden, and the body is as little affected by this intenſe degree of fire.

Mr. POTT ſays, that to melt a mixture of chalk and clay in certain proportions, which proportions appear from his tables to be equal parts, is "among the maſter-pieces of art." This mixture melts into a perfect glaſs at 123^d degrees of this thermometer.

The whole of Mr. POTT's or any other experiments may, by repeating and accompanying them with theſe thermometric pieces, have their reſpective degrees of heat aſcertained, and thereby be rendered more intelligible, and uſeful, to the reader, the experimenter, and the working artiſt.

I flatter myſelf that a field is thus opened for a new kind of thermometrical inquiries; and that we ſhall obtain clearer ideas with regard to the differences of the degrees of ſtrong fire, and their correſponding effects upon natural and artificial bodies; thoſe degrees being now rendered accurately meaſurable, and comparable with each other, equally with the lower degrees of heat which are the province of the common mercurial thermometer.

APPENDIX.

ANALYSIS OF THE CLAY OF WHICH THE THERMOMETRIC PIECES ARE FORMED.

THIS clay makes no efferveſcence with acids. Diluted nitrous and marine acids being boiled upon it, and afterwards ſaturated with fixed alkali, no precipitation or turbidneſs appeared. It therefore contains no calcareous earth, as that earth would have been diſſolved by the acids, and precipitated from them by the alkali.

Calcined with powdered charcoal, it contracted no ſulphureous ſmell, and the acids had no more action upon it than before. It therefore contains no gypſeous matter, or combination of calcareous earth with vitriolic acid; as that acid would have formed ſulphur with the inflammable principle of the charcoal, and left the calcareous earth pure, or in a ſtate of ſolubility by acids.

Some of the clay was calcined with an equal weight of ſalt of tartar, which, for the greater certainty in regard to its purity, had been run *per deliquium*, and afterwards evaporated to dryneſs. The calcined mixture was boiled in water, the filtered liquor ſlowly evaporated, and ſuffered to cool at intervals. No cryſtallization was formed; the dry ſalt appeared merely alkaline as at firſt, and deliquiated in the air; a further proof that this clay contains no gypſeous matter; for the vitriolic acid would have been abſorbed by the alkali, and formed vitriolated tartar, a ſalt which neither liquefies in the air, nor diſſolves eaſily in water, and which therefore would have cryſtallized long before the alkali became dry, or remained after its deliquiation.

A twentieth part of gypſum, ground with clay, was very diſtinguiſhable by both the foregoing proceſſes; producing a ſulphureous ſmell, and calcareous earth by calcination with charcoal powder; and cryſtals of vitriolated tartar by calcination with the ſame alkaline ſalt.

To ſeparate the pure argillaceous part, or that matter which in all clays forms alum with the vitriolic acid, 240 grains of this clay were thoroughly moiſtened with oil of vitriol, boiled to dryneſs, and at laſt made nearly red-hot. The mixture was then boiled in water; the earth which remained undiſſolved was treated again in the ſame manner with vitriolic acid, and this operation repeated five or ſix times. The clay was diminiſhed in the firſt operation about 70 grains; but leſs and leſs in the ſucceeding ones, and in the laſt ſcarcely two grains. The filtered liquors yielded cryſtals of true alum; but its quantity was not examined, as the produce of alum from aluminous earth is already ſufficiently known, and the quantity of aluminous earth itſelf, or its proportion to the indiſſoluble earth, was here the object. From the 240 grains of clay there remained in one experiment 98, and in another 95 grains of indiſſoluble earth; ſo that five parts of this clay conſiſt of three parts of pure argillaceous or alum earth, and two parts of an earth of a different kind.

With reſpect to the nature of this laſt earth, it is eaſier to determine negatively what it is not, than poſitively what it is; but aſcertaining the former will be a great ſtep towards the diſcovery of the latter.

That it is not calcareous, gypſeous, or argillaceous, is manifeſt from the experiments.—It is not jaſper; as this conſiſts, in great part, of argillaceous earth, which would have been extracted by the vitriolic acid.—It is not fluor; as this, by the ſame acid, would have been decompoſed, its own acid expelled, and a gypſeous earth left.—It is not of the micaceous kind; as the peculiar aſpect of theſe earths would readily betray them to the eye.—It is not granite; for ſtrong fire, which granite melts in, has no effect upon this.

Nor is there any known kind of earth to which it is in any degree ſimilar, except thoſe of the ſiliceous order; and with theſe it perfectly agrees in all the properties, I am acquainted with, that they poſſeſs in a ſtate of powder.

It does not vitrify or ſoften with pure clay, in the ſtrongeſt fire I have been able to produce. Nor is it diſpoſed to melt with the matter of Heſſian crucibles; for a little of it rubbed on the inſide of a crucible, and urged with ſtrong fire, continued white, powdery, and unaltered. Thirty grains of this earth were mixed with an equal weight of dry foſſil alkali, and the ſame quantity of a fine white quartzy ſand was mixed with the ſame proportion of the ſame alkali: the two mixtures were put into two ſmall crucibles, which were ſurrounded with ſand in a larger one, that both might be expoſed to an equal heat. They both began to melt at the ſame time; and at about 80° of the thermometer they had formed perfect tranſparent glaſſes.

Though theſe properties may not, perhaps, be thought ſufficient of themſelves, for determining with certainty that this ſubſtance is of the ſiliceous kind; yet, when joined to the negative proofs, of its not belonging to any other known order of earthy bodies, they afford the fulleſt evidence which the nature of the ſubject can admit of, that the indiſſoluble part of this clay is truly ſiliceous; and conſequently that the clay conſiſts of two parts of pure ſiliceous earth, to three parts of pure argillaceous or aluminous earth.

XX. *An Analyſis of Two Mineral Subſtances, viz. the* Rowley-rag-ſtone *and the* Toad-ſtone. *By* William Withering, *M. D.; communicated by* Joſeph Prieſtley, *LL. D. F. R. S. to Sir* Joſeph Banks, *Bart. P. R. S.*

Read May 16, 1782.

TO SIR JOSEPH BANKS, BART. F. R. S.

DEAR SIR,

Birmingham,
Oct. 1, 1781.

I HAVE the pleaſure to lay before you an analyſis of two mineral ſubſtances by Dr. WITHERING of Birmingham, whoſe accuracy in proceſſes of this kind will, I doubt not, give you and the members of the Royal Society great ſatiſfaction.

It may, perhaps, throw ſome additional light on the ſubject of theſe foſſils to inform you, that the Rowley-rag appears, by its texture before and after fuſion, and alſo by the quantity and quality of the air which it yields in fuſion, to be the ſame thing with the baſaltes with which you have favoured me from Scotland; and that the Toad-ſtone, treated in the ſame manner, appears (after the calcareous part has been diſſolved out of it) to reſemble ſome of the ſpecies of lava, except that it yields much more air. As Dr. WITHERING has ſent ſpecimens of the foſſils in their natural ſtate, I thought it might not be amiſs to preſent along with them the *glaſſy ſubſtances*, into which they are reduced by fuſion.

I am, with the greateſt reſpect, &c.

J. PRIESTLEY.

TO DR. PRIESTLEY.

SIR,

Birmingham,
March 28, 1781.

I NOW ſend you the reſults of my examination of the Toad-ſtone and the Rowley-rag-ſtone; being part of a plan which I have long ſince formed for a chemical analyſis of all the ſubſtances that are known to exiſt in the earth in large quantity.

Some years ago I tranſmitted to the Royal Society an analyſis of the different marles found in Staffordſhire, which they did me the honour to inſert in their Tranſactions; if they think theſe papers likewiſe worth their adoption, I ſhall ſend them the reſults of my future inquiries.

In the courſe of experiments which this ſubject has led me to, I found it convenient to form ſome new tables, and to enlarge ſome that were leſs completely formed before. Theſe tables will be uſeful in other branches of chemical inquiry. One of them I ſubjoin to the preſent papers. The facts taken from M. MACQUER are marked with an M; thoſe with the * are the conſequence of my own experiments.

In order to ſave much repetition in future, it may not be amiſs to mention, once for all, a few particulars in the conduct of theſe proceſſes.

1ſt, By *water*, is always meant water diſtilled in glaſs veſſels, or by means of a large tin refrigeratory in Mr. IRWIN's method.

2dly, Only glaſs or china veſſels are uſed in the liquid proceſſes.

3dly, By a mortar I mean thoſe excellent ones made by Mr. WEDGEWOOD; or, as will be ſpecified at the time, a ſteel mortar tempered ſo hard that it will bear the grinding of enamel in it without diſcolouration.

4thly, Filtres are never employed, it being found impoſſible to get the quantities accurate where they are uſed. The powdery parts are allowed to ſubſide until the ſupernatant liquor becomes clear. This ſometimes requires days or weeks; but I am ignorant of a better method. By giving the veſſels a circular motion round their axes, I can greatly facilitate the ſubſiding of the ſolid contents. If the ſeparating veſſels are made like a common tart-diſh, with a ſpreading border, the liquors may be poured off very near, without diſturbing the ſediments.

5thly, Phlogiſticated alkaly means the vegetable fixed alkaly prepared by the deflagration of nitre and cryſtals of tartar diſſolved in water, and boiled with Pruſſian blue in ſuch quantity that it will not any longer precipitate an earth from an acid.

I remain, &c.

W. WITHERING.

ROWLEY-RAG.

THE ſtone which is the ſubject of the following experiments forms a range of hills in the ſouthern part of Staffordſhire. The lime-ſtone rocks at Dudley bed up againſt it, and the coal comes up to the ſurface againſt the lime-ſtone. The higheſt part of the hills is near the village of Rowley. The ſummit has a craggy, broken appearance, and the fields on each ſide to a conſiderable diſtance are ſcattered over with large fragments of the rock, many of which are ſunk in the ground. In a quarry near Dudley, where a pretty large opening has been made in order to get materials for mending the roads, the rock appears to be compoſed of maſſes of irregular rhomboidal figures: ſome of theſe maſſes incloſe rounded pebbles of the ſame materials. At the diſtance of four, five, or ſix miles from the hills, as at Bilſton, Willenhall, and Wedneſbury, the Rag-ſtone is frequently found ſome feet below the ſurface in rhomboidal pieces, forming an horizontal bed of no great depth, and ſeldom of more than a few yards extent. Over the whole of this tract of country it is uſed to mend the roads, and lately has been carried to Birmingham to pave the ſtreets. Some people ſell it in powder, as a ſubſtitute for emery in cutting and poliſhing.

MORE OBVIOUS PROPERTIES.

Its appearance dark grey, with numerous minute ſhining cryſtals. When expoſed to the weather gets an ochry colour on the outſide; ſtrikes fire with ſteel; cuts glaſs; melts, though not eaſily, under the blow-pipe. Heated in an open fire becomes magnetic, and loſes about 3 in 100 of its weight.

EXPERIMENTS.

A. After three drams had been broken to ſmall pieces with a hard ſteel hammer, upon a plate of the ſame metal, it was ground to an impalpable powder in one of Mr. WEDGEWOOD's China mortars. The mortar, which had been previouſly weighed, loſt only one-third of a grain weight during this operation.

B. This powder was repeatedly waſhed with pure water, ſo as to carry off all the finer parts, and the coarſer ground again, until the whole was waſhed away. The waſhings were then filtered, and the powder carefully collected and dried. The water employed in the waſhings did not appear to have diſſolved any part of the ſtone; for no precipitate was formed either upon the addition of mild fixed alkaly, or of ſilver diſſolved in the nitrous acid.

C. 100 parts of this powder were put into a ſmall mattraſs, and covered with marine acid: a degree of heat was excited, and a very ſlight effervеſcence took place. Water was then added, and the mixture kept boiling for half an hour. The liquor was decanted off, and more acid added, which was boiled as before. This was decanted, and the reſiduum waſhed with water until the water came off taſteleſs. Theſe waters were added to the liquors before decanted. The powder had now an aſh-coloured appearance, and when dried weighed 80¼.

To the liquors (C) phlogiſticated fixed alkaly was added, until no more Pruſſian blue was precipitated. To effect this it took one ounce, five drams, and twelve grains of the phlogiſticated alkaly. The precipitate, when waſhed and dried, weighed 47.

E. The powder of 80¼ (C) mixed with twice its weight of foſſile fixed alkaly, was put into a black lead crucible, and expoſed to a red-heat for two hours. The heat was never ſufficient to render the maſs fluid, nor to make it adhere firmly to the crucible. The ſaline part was then waſhed away by repeated effuſions of hot water. To the remaining powder marine acid was added repeatedly, and boiled as before. The powder was now perfectly edulcorated by hot water, and when dry weighed 47½.

The above liquors were all added to the liquor (C), and phlogiſticated fixed alkaly was dropped in, until no more Pruſſian blue was precipitated. To effect this, half an ounce of the alkaly was required. This precipitate weighed 19; ſo that the whole of the Pruſſian blue weighed 66. After calcination in a crucible it was reduced to 31½, and was then wholly attracted by a magnet.

F. Mild fixed alkaly was now gradually added to the liquors after the ſeparation of the Pruſſian blue, and a white powder was precipitated. This powder, when well waſhed and dried, weighed 46¾. After being expoſed to a low red-heat for ten minutes, it weighed only 32¼.

G. The edulcorated powder (E) was now perfectly white; was not acted upon either by the vitriolic, nitrous, or marine acids, but readily melted into a glaſs with foſſile fixed alkaly; during the melting an efferveſcence took place.

H. The white powder (F) readily diſſolved in diluted vitriolic acid, and under a ſlow evaporation formed cryſtals which had the appearance and the taſte of allum.

Theſe cryſtals were then reduced to powder, and boiled in alcohol. The alcohol was decanted off, but did not appear to have diſſolved any part of the powder; nor did it afford any precipitate upon the addition of mild fixed alkaly.

CONCLUSIONS.

From theſe experiments it appears, that the Rowley-ragſtone conſiſts of ſiliceous earth, clay, or earth of allum, and calx of iron. From the latter muſt be deducted 11¼ for the quantity of calciform iron, found by experiment to be contained in the quantity of phlogiſticated alkaly made uſe of, and then the proportions in 100 parts of the ſtone will be theſe:

Pure

Pure ſiliceous earth -	47½
Pure clay, free from fixable air	32½
Iron in a calciform ſtate -	20
	100

From this view of the component parts of this ſtone, it is not improbable, that it might advantageouſly be uſed as a flux for calcareous iron ores. The makers of iron are acquainted with ſuch ores; but never could work them to advantage, for want of a cheap and efficacious flux.

TOAD-STONE.

FROM Derbyſhire; ſent to me by Mr. WHITEHURST, who has ſo fully and ſo accurately deſcribed the mode of its ſtratification, that it is needleſs to enlarge upon that ſubject.

MORE OBVIOUS PROPERTIES.

Of a dark browniſh grey, a granulated texture; with ſeveral cavities filled with cryſtallized ſpar. It does not ſtrike fire with ſteel. It melts to a black glaſs.

EXPERIMENTS.

A. 100 parts rubbed to an extremely fine powder in a china mortar, and boiled in marine acid; the ſolution was decanted: the undiſſolved part, after proper waſhing and drying, weighed 71.

B. The undiſſolved part was rubbed with twice its weight of mild foſſil alkaly, and then expoſed to a red-heat in a black-lead crucible for one hour.

C. This mixed maſs was reduced to powder, and repeatedly boiled, firſt in marine, afterwards in ſtrong vitriolic acid: the reſiduum now weighed 56, and was perfectly white.

D. The liquors of exp. A. and C. being put all together, phlogiſticated fixed alkaly was added until no further precipitation enſued. This precipitate was a Pruſſian blue, which, when waſhed and dried, weighed $56\frac{5}{10}$.

After expoſure to a red-heat in a crucible for forty minutes, it weighed only 29, and was wholly attracted by the magnet.

Now the 2 oz. 5 dr. and 32 gr. of phlogiſticated fixed alkaly uſed in this experiment contain 13 gr. of calciform iron, as aſcertained by a ſeparate trial; therefore, deducting 13 from 29, we have 16 for the quantity of calciform iron obtained from the ſtone.

E. The earthy parts were next precipitated from the liquors by the addition of mild foſſil alkaly. The precipitate, when perfectly edulcorated and dried, weighed $29\frac{8}{10}$.

F. Diſtilled vinegar was added to this powder, and ſuffered to ſtand in a cool place for four hours; the vinegar was poured off, and the reſiduum repeatedly waſhed with pure water. To theſe liquors mild fixed alkaly was added, and a white precipitate ſubſided, which, when waſhed and dried, weighed $7\frac{5}{10}$.

G. To the reſiduum (F) dilute vitriolic acid was added: a ſolution took place, which ſolution, by evaporation and cryſtallization, yielded allum.

H. The part of the reſiduum (F) undiſſolved by the vitriolic acid was boiled in nitrous acid, in marine acid, and in aqua regia, without being diminiſhed; the weight of it when dried was 7 $\frac{5}{10}$. It could not be fuſed by the greater heat of a blow-pipe, but melted into a glaſs when mixed with calcareous earth.

I. The undiſſolved part (exp. C.) was not fuſible by itſelf; nor was it acted on by vitriolic, nitrous, or marine acid. It melted into a glaſs with foſſil alkaly.

K. The precipitate of 7$\frac{5}{10}$ (exp. F.) after a ſufficient expoſure to heat was put into an ounce of water: the next morning the water had a pellicle upon its ſurface, and taſted like lime-water.

CONCLUSIONS.

Hence it appears, that 100 parts of this ſpecimen of Toad-ſtone contained

C. Siliceous earth	56	= 63$\frac{5}{10}$
H. More ditto	7$\frac{5}{10}$	
D. Calciform iron		16
F. K. Calcareous earth		7$\frac{5}{10}$
G. H. Earth of allum		14$\frac{8}{10}$
		101$\frac{8}{10}$

From the addition of 1$\frac{8}{10}$ of weight it is probable, that the ſubſtances capable of uniting with fixable air were not in the ſpecimen uſed fully ſaturated with it, as they would be after their precipitation by the mild alkaly.

Upon repeating theſe experiments with different portions of the Toad-ſtone, the quantities of the calcareous earth were found to differ a little; but nothing further appeared to invalidate the general concluſions.

A TABLE ſhewing the Solubility or Inſolubility of certain Saline Subſtances in Alcohol.

		Subſtances.	Reſults.
Vitriolic acid	Neutral	Vitriolated tartar	Inſoluble. M.
		Glauber's ſalt	Inſoluble. M.
		Vitriolic ammoniac.	Inſoluble. M.
	Metallic	Vitriol of ſilver	Inſoluble. M.
		——— mercury	Inſoluble. M.
		——— copper	Inſoluble. M.
		——— iron	Inſoluble. M.
		——— zinc	Inſoluble. *.
	Earthy	Heavy ſpar	Inſoluble. *.
		Selenite	Inſoluble. M.
		Allum	Inſoluble. *.
		Epſom ſalt	Soluble. *.
Nitrous acid	Neutral	Nitre	Soluble. M.
		Cubic nitre	Soluble. M.
		Nitrous ammoniac.	Soluble. M.
	Metallic	Nitre of ſilver	Soluble. M.
		——— mercury	Inſoluble. M.
		——— copper	Soluble. M.
		——— lead	Soluble. *.
	Earthy	Calcareous nitre	Soluble. M.
Muriatic acid	Neutral	Digeſtive ſalt	Soluble. M.
		Common ſalt	Inſoluble. M.
		Sal ammoniac.	Soluble. M.
	Metallic	Luna cornea	Inſoluble. M.
		Corroſ. Sublimate	Soluble. M.
		Muria cupri	Soluble. M.
		——— ferri	Soluble. M.
	Earthy	Muria calcarea	Soluble. M.
		——— magneſiæ	Soluble. *.
		——— aluminoſa	Soluble. *.
Vegetable acid	Neutral	Soluble tartar	Soluble. *.
		Rochelle ſalt	Inſoluble. *
		Veget. ammoniac.	
	Metallic	Verdigraſs	Soluble. *.
		Sugar of lead	Soluble. *.
Calcareous acid	Neutral	Veg. alkaly mild	Inſoluble. *
		Foſſ. alkaly mild	Inſoluble. *
		Vol. alkaly mild	Soluble. *.
		Calcareous ſpar	Soluble. *.

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XXI. *New Fundamental Experiments upon the Colliſion of Bodies.* *By Mr.* John Smeaton, *F. R. S. in a Letter to Sir* Joſeph Banks, *Bart. P. R. S.*

Read April 18, 1782.

TO SIR JOSEPH BANKS, BART. P. R. S.

SIR,

THE ſubjects of the incloſed tract have been the object of my conſideration for many years paſt; and as they contain ſome matters that have not only been variouſly reaſoned about, but variouſly concluded upon; if what is contained therein ſhall appear of ſuch a nature as either to eſtabliſh truth, as it appears to me; or to prompt ſome more able perſon, in reviewing the ſubject, to ſhew what links in my chain of reaſoning thereon are defective, ſo as to eſtabliſh the whole doctrine of moving bodies upon one plain conſiſtent baſis, my end will be equally anſwered in offering them to you, to be laid before the Royal Society, in caſe you ſhall think that the importance of the ſubject ſhall merit the ſame: furthermore, I hope to be forgiven, if in ſome parts of this paper I have expreſſed myſelf with more pointedneſs than I might have done, for I declare, that it was ſolely owing to my earneſtneſs that the ſubject of mechanic motions and powers ſhould be fully and freely inveſtigated, and eſtabliſhed upon grounds that ſhall be uncontrovertible.

I have the honour to be, &c.

Gray's-Inn,
April 18, 1782.

IT is univerſally acknowledged, that the firſt ſimple principles of ſcience cannot be too critically examined, in order to their being firmly eſtabliſhed; more eſpecially thoſe which relate to the practical and operative parts of mechanics, upon which much of the active buſineſs of mankind depends. A ſentiment of this kind occaſioned my tract upon *Mechanic Power*, which was publiſhed in the Philoſophical Tranſactions, vol. LXVI. for the year 1776. What I have now to offer was intended as a ſupplement thereto, and the experiments were then, in part, tried; but the completion thereof was deferred at that time, partly from want of leiſure; partly to avoid too great a length of the paper itſelf; and partly to avoid the bringing forward too many points at once. My preſent purpoſe is to ſhew, that the true doctrine of the *colliſion of bodies* hangs as it were upon the ſame hook, as the doctrine of the gradual generation of motion from reſt, conſidered in that paper; that is, that whether bodies are put into gradual motion, and uniformly accelerated from reſt to any given velocity; or are put in motion, in an inſtantaneous manner, when bodies of any kind ſtrike one another; the motion, or ſum of the motions produced, has the ſame relation to mechanic power therein defined, which is neceſſary to produce the motion deſired. To prove this, and at the ſame time to ſhew ſome capital miſtakes in principle, which have been *aſſumed* as indiſputable truths by men of great learning, is the reaſon of my now purſuing the ſame ſubject.

I do not mean to point out the particular miſtakes which have been made by particular men, as that would lead me into too great a length: I ſhall therefore content myſelf with obſerving, that the laws of colliſion, which have been inveſtigated by mathematical philoſophers, are principally of three kinds; *viz.* thoſe relating to bodies perfectly *elaſtic*; to bodies perfectly unelaſtic, and perfectly *ſoft*; and to bodies perfectly unelaſtic, and perfectly *hard.* To avoid prolixity, I ſhall conſider in each, only the ſimple caſe of two bodies which are equal in weight or quantity of matter ſtriking one another. Reſpecting thoſe which are perfectly elaſtic, it is univerſally agreed, that when two ſuch bodies ſtrike one another, no motion is loſt; but that in all caſes, what is loſt by one is acquired by the other: and hence, that if an elaſtic body in motion ſtrikes another at reſt, upon the ſtroke the former will be reduced to a ſtate of reſt, and the latter will fly off with an equal velocity.

In like manner, if a non-elaſtic *ſoft* body ſtrikes another at reſt, they neither of them remain at reſt, but proceed together from the point of colliſion with exactly one half of the velocity that the firſt had before the ſtroke; this is alſo univerſally allowed to be true, and is fully proved by every good experiment upon the ſubject.

Reſpecting the third ſpecies of body, that is, thoſe that are non-elaſtic, and yet perfectly hard; the laws of motion relating to them, as laid down by one ſpecies of philoſophers, have been rejected by another; the latter alledging, that there are no ſuch bodies to be found in nature whereon to try the experiment; but thoſe who have laid down and aſſigned the doctrine that would attend the colliſion of bodies of this kind (if they could be found) have univerſally agreed, that if a non-elaſtic *hard* body was to ſtrike another of the ſame kind at reſt, that, in the ſame manner as is agreed concerning non-elaſtic ſoft bodies, they neither of them would remain at reſt, but would in like manner proceed from the point of colliſion, with exactly one half of the velocity that the firſt had before the ſtroke: in ſhort, they lay it down as a rule attending all non-elaſtic bodies, whether hard or ſoft, that the velocity after the ſtroke will be the ſame in both, *viz. one half* of the velocity of the original ſtriking body.

Here is therefore the aſſumption of a principle, which in reality is proved by no experiment, nor by any fair deduction of reaſon that I know of, *viz.* that the velocity of non-elaſtic *hard* bodies after the ſtroke muſt be the ſame as that reſulting from the ſtroke of non-elaſtic *ſoft* bodies; and the queſtion now is, whether it is true or not?

Here it may be very properly aſked, what ill effects can reſult to practical men, if philoſophers ſhould reaſon wrong concerning the effects of what does not exiſt in nature, ſince the practical men can have no ſuch materials to work upon, or misjudge of? But it is anſwered, that they who infer an equality of effects between the two ſorts, may from thence be miſled themſelves, and in conſequence miſlead practical men in their reaſonings and concluſions concerning the ſort with which they have abundant concern, to wit, the non-elaſtic *ſoft bodies*, of which water is one, which they have much to do with in their daily practice.

Previous to the trying my experiment on mills I never had doubted the truth of the doctrine, that the ſame velocity reſulted from the ſtroke of both ſorts of non-elaſtic bodies; but the trial of thoſe experiments made me clearly ſee at leaſt the inconcluſiveneſs, if not the falſity of that doctrine: becauſe I found a reſult which I did not expect to have ariſen from either ſort; and for the which, when it appeared from experiment, I could ſee a ſubſtantial reaſon why it ſhould take place in one ſort, and that it was impoſſible that it could take place in the other; for if it did, the bodies could not have been perfectly *hard*, which would be contrary to the hypotheſis. Of this deduction I have given notice in my ſaid tract on mills, publiſhed in the Philoſophical Tranſactions, vol. LI. for the year 1759 *.

It may alſo be ſaid, that ſince we have no bodies perfectly elaſtic, or perfectly unelaſtic and *ſoft*, why ſhould we expect and bodies perfectly unelaſtic and *hard*? Why may not the effects be ſuch as ſhould reſult from a ſuppoſition of their being *imperfectly elaſtic* joined with their being *imperfectly hard*? But here I muſt obſerve, that the ſuppoſition appears to be a contradiction in terms.

We have bodies which are ſo nearly perfectly elaſtic, that the laws may be very well deduced and confirmed by them; and the ſame obtains with reſpect to non-elaſtic *ſoft* bodies; but concerning bodies of a mixed nature, which are by far the greateſt number, ſo far as they are wanting in elaſticity, they *are ſoft*, and *bruiſe*, *yield*, or *leave a mark* in colliſion; and ſo far as they are not perfectly ſoft they are elaſtic, and obſerve a mixture of the law relative to each; but imperfectly elaſtic bodies, imperfectly hard come in reality under the *ſame deſcription* as the former mixed bodies: for ſo far as they are imperfectly hard they are ſoft, and either *bruiſe* and *yield*, or leave a mark in the ſtroke; and ſo far as they want perfect elaſticity, they are non-elaſtic; that is to ſay, they are bodies imperfectly elaſtic, and imperfectly ſoft; and in fact I have never yet ſeen any bodies but what come under this deſcription. It ſeems, therefore, that reſpecting the *hardneſs* of bodies they differ in degrees of it, in proportion as they have a greater degree of tenacity or coheſion; that is, are further removed from perfect ſoftneſs, at the ſame time that their elaſtic ſprings, ſo far as they reach, are very ſtiff; and hence we may (by the way) conclude, that the ſame mechanic power that is required to change the figure in a *ſmall degree* of thoſe bodies that have the popular appellation of *hard bodies*, would change it in a *great degree* in thoſe bodies that approach towards ſoftneſs, by having a ſmall degree of tenacity or coheſion. In the former kind we may rank the harder kinds of *caſt iron*, and in the latter, *ſoft tempered clay*.

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* "The effect, therefore, of overſhot wheels, under the ſame circumſtance of "quantity and fall, is at a medium double to that of the underſhot: and as a "conſequence thereof, that *non-elaſtic bodies, when acting by their impulſe or colliſion,* "*communicate only a part of their original power*; the other part *being ſpent in* "*changing their figure in conſequence of the ſtroke.*" Phil. Tranſ. vol. LI. p. 133.

While the philoſophical world was divided by the diſpute about the *old* and *new opinion*, as it was called, concerning the powers of bodies in motion, in proportion to their different velocities: thoſe who held the old opinion contending, that it was as the velocity *ſimply*, aſked thoſe of the new, How, upon their principles, they would get rid of the concluſions ariſing from the doctrine of unelaſtic perfectly hard bodies? They replied, They found no ſuch bodies in nature, and therefore did not concern themſelves about them. On the other hand, thoſe of the new opinion aſked thoſe of the old, How they would account for the caſe of non-elaſtic ſoft bodies, where, according to them, the whole motion loſt by the ſtriking body was retained in the two after the ſtroke (the two bodies moving together with the half velocity), though the two non-elaſtic bodies had been bruiſed and changed their figure by the ſtroke; for, if no motion was loſt, the change of figure muſt be an effect without a cauſe? To obviate this, thoſe of the old opinion ſeriouſly ſet about proving, that the bodies might change their figure, without any loſs of motion in either of the ſtriking bodies.

Neither of theſe anſwers have appeared to me ſatisfactory, eſpecially ſince my mill experiments: for with reſpect to the firſt, it is no proper argument to urge the impoſſibility of finding

ing the proper material for an experiment, in anſwer to a concluſion drawn from an abſtract idea. On the other hand, if it can be ſhewn, that the figure of a body can be changed, without a *power*, then, by the ſame law, we might be able to make a *forge hammer* work upon a maſs of ſoft iron, without any other power than that neceſſary to overcome the friction, reſiſtance, and original *vis inertiæ*, of the parts of the machine to be put in motion: for, as no progreſſive motion is given the maſs of iron by the hammer (it being ſupported by the anvil), no power can be expended that way; and if none is loſt to the hammer from changing the figure of the iron, which is the only effect produced, then the whole power muſt reſide in the hammer, and it would jump back again to the place from which it fell, juſt in the ſame manner as if it fell upon a body perfectly elaſtic, upon which, if it did fall, the caſe would really happen: the power, therefore, to work the hammer would be the ſame, whether it fell upon an elaſtic or non-elaſtic body; an idea ſo very contrary to all experience, and even apprehenſion, of both the philoſopher and vulgar artiſt, that I ſhall here leave it to its own condemnation.

As nothing, however, is ſo convincing to the mind as experiments obvious to the ſenſes, I was very deſirous of contriving an experiment in point; and as I ſaw no hopes of finding matter to make a *direct* experiment, I turned my mind towards an indirect one; ſo circumſcribed, however, as to prove inconteſtably, that the reſult of the ſtroke of two non-elaſtic perfectly hard bodies could not be the ſame as would reſult from the colliſion of two ſoft ones; that is, if it can be *bona fide* proved, that *one half* of the original power is loſt in the ſtroke of ſoft bodies by the change of figure (as was very ſtrongly ſuggeſted by the mill experiments); then ſince no ſuch loſs can happen in the colliſion of bodies perfectly hard, the reſult and conſequence of ſuch a ſtroke muſt be *different*.

The conſequence of a ſtroke of bodies perfectly hard, but void of elaſticity, muſt doubtleſs be different from that of bodies perfectly *elaſtic*: for having no ſpring the body at reſt could not be driven off with the velocity of the ſtriking body, for that is the conſequence of the action of the ſpring or elaſtic parts between them, as will be ſhewn in the reſult of the experiments; the ſtriking body will therefore not be ſtopped, and as the motion it loſes muſt be communicated to the other, from the equality of action and re-action, they will both proceed together, with an equal velocity, as in the caſe of non-elaſtic ſoft bodies: the queſtion, therefore, that remains is, what that *velocity muſt be?*—It muſt be greater than that of the non-elaſtic ſoft bodies, becauſe there is no mechanical power loſt in the ſtroke. It muſt be leſs than that of the ſtriking body, becauſe, if equal, inſtead of a *loſs* of motion by the colliſion, it will be doubled. If, therefore, non-elaſtic ſoft bodies loſe half their motion, or mechanical power, by change of figure in colliſion, and yet proceed together with half the velocity, and the non-elaſtic hard bodies can loſe *none* in any manner whatever; then, as they muſt move together, their velocity muſt be ſuch as to preſerve the equality of the mechanic power, *unimpaired*, after the ſtroke the ſame as it was before it.

For example, let the velocity of the ſtriking body before the ſtroke be 20, and its maſs or quantity of matter 8; then, according to the rule deduced from the experiments in the tract on *Mechanic Power* (ſee exp. third and fourth) that power will be expreſſed by 20 × 20 = 400, which × 8 = 3200; and if half of it is loſt in the ſtroke, in the caſe of non-elaſtic ſoft bodies, it will be reduced to 1600; which ÷ 16 the double quantity of matter, will give 100 for the ſquare of their velocity; the ſquare root of which being 10, will be the velocity of the two non-elaſtic ſoft bodies after the ſtroke, being juſt one half of the original velocity, as it is conſtantly found to be. But in the non-elaſtic hard bodies, no power being loſt in the ſtroke, the mechanic power will remain after it, as before it, = 3200; this, in like manner, being divided by 16, the double quantity of matter, will give 200 for the ſquare of the velocity, the ſquare root of which is 14. 14, &c. for their velocity after the ſtroke, which is to 10, the velocity of the non-elaſtic ſoft bodies after the ſtroke, as the ſquare root of 2 to 1; or as the diagonal of a ſquare to its ſide.

It remains, therefore, now to be proved, that preciſely half of the mechanic power is *loſt* in the colliſion of non-elaſtic ſoft bodies; for which purpoſe my mind ſuggeſted the following reflections. In the colliſion of elaſtic bodies the effect ſeemingly inſtantaneous, is yet performed in *time*; during which time the natural ſprings reſiding in elaſtic bodies, and which conſtitute them ſuch, are bent or forced, till the motion of the ſtriking body is divided between itſelf and the body at reſt; and in this ſtate the two bodies would then proceed together, as in the caſe of non-elaſtic ſoft bodies; but as the ſprings will immediately reſtore themſelves in an equal time, and with the ſame degree of *impulſive force*, wherewith they were bent in this re-action, the motion that remained in the ſtriking body will be totally deſtroyed, and the total exertion of the two ſprings, communicated to the original reſting body, will cauſe it to fly off with the ſame velocity wherewith it was ſtruck.

Upon this idea, if we could conſtruct a couple of bodies in ſuch a way that they ſhould either act as bodies perfectly elaſtic; or, that their ſprings ſhould at pleaſure be hooked up, retained, or prevented from reſtoring themſelves, when at their extreme degree of bending; and if the bodies under theſe circumſtances obſerved the laws of colliſion of non-elaſtic ſoft bodies, then it would be proved, that one half of the mechanical power, reſiding in the ſtriking body, would be loſt in the action of colliſion; becauſe the impulſive force or power of the ſpring in its reſtitution being cut off, or ſuſpended from acting, which is equal to the impulſive force or power to bend it (and which alone has been employed to communicate motion from one body to the other), it would make it evident, that one half of the impulſive force is loſt in the action, as the other half remains *locked up* in the ſprings. It alſo follows, as a *collateral circumſtance*, that be the impulſive power of the ſprings what it may from firſt to laſt, yet as one half of the *time* of the action is by this means cut off, in this ſenſe alſo it will follow, that one half of the mechanic power is deſtroyed; or rather, in this caſe, remains locked up in the ſprings, capable of being *re-exerted* whenever they are ſet at liberty, and of producing a freſh mechanical effect, equivalent to the motion or mechanical power of the two non-elaſtic ſoft bodies after their colliſion.

Hence we muſt infer, that the quantity of mechanical power expended in diſplacing the parts of non-elaſtic ſoft bodies in colliſion, is exactly the ſame as that expended in bending the ſprings of perfectly elaſtic bodies; but the difference in the ultimate effect is, that in the non-elaſtic ſoft bodies, the power taken to diſplace the parts will be totally loſt and deſtroyed, as it would require an equal mechanic power to be raiſed a-freſh, and exerted in a contrary direction to reſtore the parts back again to their former places; whereas, in the caſe of the elaſtic bodies, the operation of half the mechanic power is, as obſerved already, only locked up and ſuſpended, and capable of being re-exerted without a further original acceſſion.

Theſe ideas aroſe from the reſult of the experiments tried upon the machine deſcribed in my ſaid tract upon Mechanic Power, and were alſo communicated to my very worthy and ingenious friend WILLIAM RUSSELL, Eſq. F. R. S. at the ſame time that I ſhewed him thoſe experiments in 1759; but the mode of putting this matter to a full and fair mechanical trial has ſince occurred; and though ſome rough trials, ſufficient to ſhew the effect, were made thereon, prior to the offering the paper on mechanical power to the Society in 1776, yet the machine itſelf I had not leiſure to complete to my ſatisfaction till lately; which I mention to apologize for the length of time that theſe ſpeculations have taken in bringing forward.

DESCRIPTION OF THE MACHINE FOR COLLISION.

Fig. 1. ſhews the front of the machine as it appears at reſt when fitted for uſe.

A is the pedeſtal, and AB the pillar, which ſupports the whole, C, D are two compound bodies of about a pound weight each, but as nearly equal in weight as may be. Theſe bodies are alike in conſtruction, which will be more particularly explained by fig. 2. Theſe bodies are ſuſpended by two white fir rods of about half an inch diameter *ef* and *gh*, being about four feet long from the point of ſuſpenſion to the center of the bodies; and their ſuſpenſion is upon the croſs-piece II, which is mortoiſed through, to let the rods paſs with perfect freedom; and they hang upon two ſmall plates filed to an edge on the under ſide, and paſs through the upper part of the rods. Their centers are at *k* and *l*, and the edges being let into a little notch, on each ſide the mortoiſe, the rods are at liberty to vibrate freely upon their reſpective points (or rather edges) of ſuſpenſion, and are determined to one plain of vibration. MN is a flat arch of white wood, which may be covered with

paper,

Paper, that the marks thereupon may be the more conſpicuous.

The croſs-piece II is made to project ſo far before the pillar, that the bodies in their vibrations may paſs clear of it, without danger of ſtriking it; and alſo the arch MN is brought ſo far forward as to leave no more than a clearance, ſufficient for the rods to vibrate freely without touching it.

Fig. 2. Shews one of the compound bodies, drawn of its full ſize. AB is a block of wood, and about as much in breadth as it is repreſented in height, through a hole in which the wood rod CC paſſes, and is fixed therein.

DB repreſents a plate of lead about three-eighths of an inch thick, one on each ſide, ſcrewed on by way of giving it a competent weight. *d*B*efg* repreſents the edge of a ſpringing plate of braſs, rendered elaſtic by hard hammering; it is about five-eighths of an inch in breadth, and about one-twentieth of an inch thick. It is fixed down upon the wooden block at its end *d*B by means of a bridge plate, whoſe end is ſhewn *hi*, and is ſcrewed down on each ſide the ſpring plate by a ſcrews which being relaxed the ſpring can be taken out at pleaſure, and adjuſted to its proper ſituation. *kl* is a light thin ſlip of a plate, whoſe under edge is cut into teeth like a fine ſaw or ratchet, and is attached to the ſpring by a pin at *k*, which paſſes through it, and alſo through a ſmall ſtud rivetted into the back part of the ſpring, and upon which pin, as a center, it is freely moveable.

mn ſhews a ſmall plate or ~~ſtud~~ ſeen edgeways raiſed upon the bridge plate, through an hole in which ſtud the ratchet paſſes; and the lower part of the hole is cut to a tooth ſhaped properly to catch the teeth of the ratchet, and retain it together with the ſpring at any degree to which it may be ſuddenly bent; and for this intent it is kept bearing gently downward, by means of a wire-ſpring *opq*, which is in reality double, the bearing part at *o* being ſemi-circular; from which branching off on each ſide the rod *cc*, paſſes to *p*, and fixes at each end into the wood at *q*. However, to clear the ratchet, which is neceſſarily in the middle as well as the rod, the latter is perforated; and alſo the block is cut away, ſo far as to ſet the main ſpring at *e* free of all obſtacles that would prevent its play from the point B. The part *fg* is ſhewn thicker than the reſt, by being covered with thin kid leather tight ſowed on, to prevent a certain jarring that otherwiſe takes place on the meeting of the ſprings in colliſion.

Let us now return to fig. 1. the marks upon the arch MN are put on as follows. *op* is an arch of a circle from the center *l*, and *qr* an arch of a circle from the center *k* interſecting each other at *s*. Now the middle line of the marks *t*, *v*, are at the ſame diſtance from the middle line at *s* that the centers *kl* are; ſo that when each body hangs in its own free poſition, without bearing againſt the other, the rod *ef* will cover the mark at *t*, and the rod *gh* will cover the mark at *v*. From the point S upon the arches S*p* and S*q* reſpectively, ſet off points at an equal and competent diſtance from S each way, which will give the middle of the mark *w* and *x*: and upon the arch S*p* find a middle point between the mark *v* and *w*, which let be *y*; and on the other ſide, in like manner, upon the arch S*q* find a middle point for the mark *z*; then ſet off the diſtance S*v* or S*t* from *y* each way, and from *z* each way; and from theſe points, drawing lines to the reſpective centers *l* and *k*, they will give the place and poſition of the marks *a*, *b*, and *c*, *d*; and thus is the machine prepared for uſe.

FOR TRIALS ON ELASTIC BODIES.

For this uſe take out the pins and ratchets from each reſpectively, and the ſprings being then at liberty, with a ſhort bit of ſtick (ſuppoſe the ſame ſize as the rods) turn aſide the rod *gh* with the right-hand, carrying the body D upwards till the ſtick is upon the mark *w*, as ſuppoſe at o; there hold it, and with the left ſet the body C perfectly at reſt; in which caſe the rod *ef* will be over the mark *t*; then ſuddenly withdraw the ſtick, in the direction that the rod *gh* is to follow it, and the ſpring of the body D, impinging upon that of the body C, they will be both bent, and alſo reſtored; and the body C will fly off, and mount till its rod *ef* covers the mark *x*; the rod of the ſtriking body D remaining at reſt upon its proper mark of reſt *v*, till the body C returns, when the body D will fly off in he ſame manner; the two bodies thus rebounding a number of times, loſing a part of their vibration each time; but ſo nearly is the theory of elaſtic bodies fulfilled hereby, that the ſingle advantage of originally puſhing the rod *gh* beyond the mark *w*, by the thickneſs of the ſtick, or its own thickneſs, is ſufficient to carry the rod of the quieſcent body C completely to its mark *x*.

There are ſeveral other experiments that may be made with this apparatus, in confirmation of the doctrine of the colliſion of elaſtic bodies; which being univerſally agreed upon, and well known, it is needleſs further to dwell upon here; but reſpecting the application to non-elaſtic ſoft bodies, it is far more difficult to come at a fitneſs of materials for this kind of experiments, than it is for thoſe ſuppoſing perfectly elaſticity. The concluſions, however, may be attained with equal certainty.

FOR TRIALS ON NON-ELASTIC SOFT BODIES.

For this purpoſe the ratchets muſt be applied and put in order as before deſcribed, and the ſprings being both put to their point of reſt, let the body D be put to its mark *w* in the ſame manner as before deſcribed, and the body C to reſt. The body D being let go, and ſtriking the body C at reſt, in conſequence of the ſtroke, the ſprings being hooked up by the ratchets, they both move from their reſting marks *t*, *v*, reſpectively toward M: Now if they both moved together, and the rod *ef* covered the mark *c*, and the rod *gh* covered the mark *d* at their utmoſt limit, then they would truly obey the laws of non-elaſtic ſoft bodies; becauſe their medium aſcent would be to the mark *z*, which is juſt half the angle of aſcent to the mark *x*; but as in this piece of machinery, though the main or principle ſprings are hooked up, yet every part of them, and all the materials of which they are compoſed, and to which they are attached, have a degree, or more properly ſpeaking, a certain compaſs of *elaſticity*, which, as ſuch, is perfect, and no motion loſt thereby.

We muſt not, therefore, expect the two compound bodies after the ſtroke to ſtick together without ſeparating, as would be the caſe with bodies truly non-elaſtic and ſoft; but that from the elaſticity they are poſſeſſed of, they will by rebounding be ſeparated; but that elaſticity being perfect, can occaſion no loſs of motion to the ſum of the two bodies; ſo that if the body C aſcends as much above its mark *c* as the body D falls ſhort of its mark *d*, then it will follow, that their medium aſcent will ſtill be to the mark *z*, as it ought to have been, had theybeen truly non-elaſtic ſoft bodies; and this, in reality, is truly the caſe in the experiment, as nearly as it can be diſcerned.

After a few vibrations, by the rubbing of the ſprings againſt one another, they are ſoon brought to reſt; and here they would *always reſt* had they been truly and properly perfect non-elaſtic ſoft bodies; but here, as in the caſe of theſe bodies, by a change of the figure and ſituation of the component parts, there is expended one half of the mechanical power of the firſt mover, yet in this caſe the other half is not *loſt*, but *ſuſpended*, ready to be re-exerted whenever it is ſet at liberty; and that it is really and *bona fide one half* and neither more or leſs, appears from this uncontroverted ſimple principle, that the power of reſtitution of a perfect ſpring is exactly equal to the power that bends it. And this may, in a certain degree, be ſhewn to be fact by experiment, if there were any need of ſuch a proof; for if, when the bodies are at reſt after the laſt experiment, the two rods are laſhed together near the bottom with a bit of thread, and then the ratchets unpinned and removed; on cutting the thread with a pair of ſciſſars they will each of them rebound, C towards M, and D towards N; and if they rebounded reſpectively to *x* and *y*, the mechanical power exerted would be the ſame as it was after the ſtroke, when the mean of their two aſcents was up to the mark *z*; but here it is not to be expected, becauſe not only the motion loſt by the friction of the ratchets is to be deducted, becauſe it had the effect of real non-elaſticity; but alſo the elaſticity that ſeparated them in the ſtroke, which was loſt in the vibrations that ſucceeded; neither of which hindered the mean aſcent to be to *z*; but yet, under all theſe diſadvantages in the machine (if not unreaſonably ill made) the rod *ef* will aſcend to *d*, and *gh* to *a*: and hence I infer, as a poſitive truth, that in the colliſion of non-elaſtic ſoft bodies, *one half of the mechanic power reſiding in the ſtriking body is loſt in the ſtroke.*

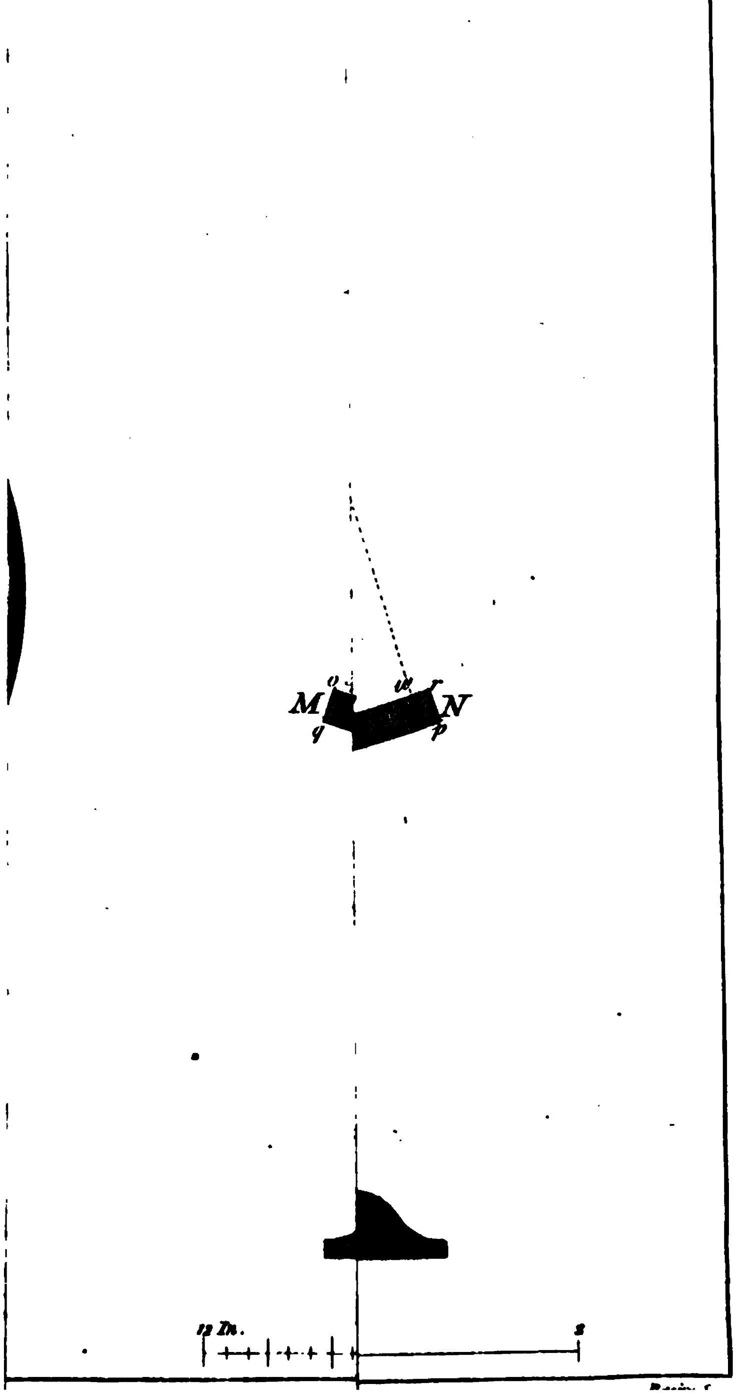
Reſpecting bodies unelaſtic and perfectly hard, we muſt infer, that ſince we are unavoidably led to a concluſion concerning them, which contradicts what is eſteemed a truth capable of the ſtricteſt demonſtration; *viz.* that the velocity of the center of gravity of no ſyſtem of bodies can be changed by any colliſion betwixt one another, ſomething muſt be aſſumed that involves a contradiction. This perfectly holds, according to all the eſtabliſhed rules, both of perfectly elaſtic and perfectly non-elaſtic *ſoft* bodies; rules which muſt fail in the perfectly non-elaſtic *hard* bodies, if their velocity after the ſtroke is to the velocity of the ſtriking body as one is to the ſquare root of 2; for then the center of gravity of the two bodies will by the ſtroke acquire a velocity greater than the center of gravity the two bodies had before the ſtroke in that proportion, which is proved thus.

At the outſet of the ſtriking body, the center of gravity of the two bodies in our caſe will be exactly in the middle between the two; and when they meet it will have moved from their half diſtance to their point of contact, ſo the velocity of the center of gravity before the bodies meet will be exactly one half of the velocity of the ſtriking body; and, therefore, if the velocity of the ſtriking body is 2, the velocity of the center of gravity of both will be one. After the ſtroke, as both bodies are ſuppoſed to move in contact, the velocity of the center of gravity will be the ſame as that of the bodies; and as their velocity is proved to be the ſquare root of 2, the velocity of their center of gravity will be increaſed from 1. to the ſquare root of 2.; that is, from 1. to 1.414, &c.

The fair inference from theſe contradictory concluſions therefore is, that an unelaſtic hard body (perfectly ſo) is a repugnant idea, and contains in itſelf a contradiction; for to make it agree with the fair concluſions that may be drawn on each ſide, from clear premiſes, we ſhall be obliged to define its properties thus: that in the ſtroke of unelaſtic hard bodies they cannot *poſſibly loſe any* mechanic power in the ſtroke; becauſe no other impreſſion is made than the communication of motion; and yet they *muſt loſe* a *quantity* of mechanic power in the ſtroke; becauſe, if they do not, their common center of gravity, as above ſhewn, will acquire an *increaſe* of velocity by their ſtroke upon each other.

In a like manner the idea of a *perpetual motion*, perhaps, at firſt ſight, may not appear to involve a contradiction in terms; but we ſhall be obliged to confeſs that it does, when, on examining its requiſites for execution, we find we ſhall want bodies having the following properties; that when they are made to *aſcend* againſt gravitation their abſolute weight ſhall be *leſs*; and that when they *deſcend* by gravitation (through an equal ſpace) their abſolute weight ſhall be greater; which, according to all we know of nature, is a *repugnant* or *contradictory idea*.

or COLLISION.



XXII. *Proceedings relative to the Accident by Lightning at* Heckingham.

LETTER FROM THE BOARD OF ORDNANCE.

SIR,

HAVING received information that, laſt ſummer, a ſtroke of lightning ſet fire to the Poor-houſe at Heckingham, near Norwich, notwithſtanding it was armed with *eight pointed conductors*, we requeſt you will communicate to us ſuch particulars relating to that fact, as may have come to your knowledge.

We are, with great reſpect,

SIR,

Your moſt obedient humble ſervants,

AMHERST.
CHARLES FREDERICK.
H. STRACHEY.
J. KENRICK.

Office of Ordnance,
22d December, 1781.

Sir Joſ. Banks, Bart. Preſident of the Royal Society.

Extracts from the Minutes of the Council of the Royal Society.

January 10, 1782.

THE Preſident laid before the Council a letter to him from the Board of Ordnance, acquainting him, that the Poor-houſe at Heckingham, near Norwich, had been ſtruck by lightning, notwithſtanding it was armed with eight pointed conductors; and requeſting him to communicate to them ſuch particulars relating to that fact as may have come to his knowledge.

Reſolved,

That Dr. Blagden and Mr. Nairne be requeſted to repair to Heckingham, and examine into the circumſtances of the accident, and report thereon to the Council: that they engage a draughtſman, to take ſuch drawings as may be requiſite; and that the neceſſary expences be defrayed by the Society.

February 7, 1782.

Dr. Blagden read to the Council his and Mr. Nairne's Report of the Survey made by them of the Poor-houſe at Heckingham in Norfolk, in conſequence of their appointment by a former Council. The ſaid Report was ordered to be read to the Society on Thurſday the 14th inſtant. And the Preſident was requeſted to tranſmit it immediately afterwards to the Board of Ordnance; and to deſire that they would return the drawings as ſoon as they ſhould have taken copies of them, or made ſuch other uſe of them as they might think neceſſary.

Report of the Committee.

Read February 14, 1782.

To the Preſident and Council of the Royal Society.

GENTLEMEN,

PURSUANT to your reſolution, appointing us a committee to examine the Houſe of Induſtry at Heckingham in Norfolk, which had been ſtruck by lightning although it was armed with conductors, we arrived there on the 21ſt of January. Seven months had then elapſed ſince the accident, yet we had the ſatisfaction to learn, that no material changes had been made in the conductors or the building in that period; ſome laths that had been burnt, ſome bricks and pantiles which had been damaged or thrown down, were replaced; but we found means to procure diſtinct information of thoſe repairs from the workmen who had been employed to execute them. In order to communicate a clear idea of the accident, it will be neceſſary to premiſe a general account of the building; then to repreſent the manner in which the conductors were applied; and, laſtly, to deſcribe the ſtroke of lightning, with its effects.

The general form of the building is that of the Roman letter H (ſee the general plan, fig. 1.), conſiſting of a center range (Z) and two flanks (Y and X). It ſtands on a gentle riſing, which can by no means be termed a hill, with its front facing S. 9° W. To the weſtern ſide of the weſt flank, and eaſtern ſide of the eaſt flank, ſome lower buildings are annexed, ſerving as offices of different kinds; and there are two courts, one before and the other behind the houſe, together with ſome ſmall gardens and yards on each of the flanks, in all of which ſtand various detached offices, as will be eaſily conceived from the general plan (fig. 1.).

The body of the building, including the great houſe with its annexed offices, is provided with eight chimnies, the poſition of which is repreſented in the plan (fig. 1.) at the letters A, B, C, D, E, F, G, H. Of theſe the ſix firſt are all placed on the ridges of the roof; namely, A and B on the ridge of the weſt flank, C and D on the ridge of the center range, and E and F on the ridge of the eaſt flank; but the chimney G riſes from the lower part of the roof on the eaſtern ſide of the eaſt flank; and the chimney H from the roof of an annexed office, the boiling-room, which roof is continued down from the general roof, and projects beyond it.

Both flanks (X and Y, fig. 1.) at their north and ſouth ends are hipped off from the ridge of the roof to the eaves on each ſide; conſequently there are eight hips, all of which are covered or *coped* with lead; the four vallies alſo, formed by the interſection of the center range with the two flanks (ſee fig. 1.) are in like manner covered with lead, which here anſwers the purpoſe of a ſpout. (Two of theſe hips are ſhewn in the 2d, 3d, 4th, and 11th figures at *h*, *h*, and one of the vallies at *v*, fig. 2.) Thoſe twelve ſtrips of lead, covering the hips and the vallies (ſee the general plan, fig. 1.), are all ſeparate, not having any metalline communication with one another, as the reſt of the roof conſiſts merely of pantiles, with dropping eaves.

From the ſouth-eaſt corner of the eaſt flank a wall is continued eaſtward (ſee I in the 1ſt and 3d figures) above 26 feet in length, having a ſmall garden at its ſouth ſide, and on the north a ſtable built againſt it as a *lean-to* (K in the 1ſt and 4th figures); which ſtable is alſo ſupported on the weſt by the eaſt wall of the eaſt flank of the building (fig. 1. at *t*). The roof of the ſtable being like that of a ſhed, ſlopes downward from ſouth to north (*a* and *b* in the 4th and 11th figures); it does not reach up quite to the top of the wall againſt which it reſts to the ſouthward, but is ſhorter by one courſe of bricks on edge (ſee fig. 10. and 11. at *c*); and at its junction with the wall a *flaſhing* of lead is carried along horizontally (from *c* to *d*, fig. 11.) 25½ feet in length.

We conclude this general account of the building with the dimenſions of its principal parts.

	Ft.	In.
Length of the center range (Z fig. 1.) to the flanks - - - - -	108	9
Length of each flank (X and Y fig. 1.) -	159	7
Breadth of the center range, and of each flank,	31	4
Height from the ground to the bottom of the hips (*g*, *g*, fig. 3. and 4.) - - about	20	6
Height from the ground to the top of the ridge (*f*; fig. 2. and 3. *e*, fig. 4.) - about	34	0
Height of the chimnies above the ridge of the roof (as E, fig. 4.) - - about	3	6
Length of each hip (from *f* to *g*, fig. 3. 4. and 11.) - - - about	27	0
Height of the wall (l) ſupporting the ſtable (fig. 3.)	16	0
Height of the eaves of the ſtable above the garden to the northward of it (ſee general plan, fig. 1.)	7	0
Length of the ſtable on the outſide -	26	2
Breadth of the ſtable on the outſide -	15	5

To all the eight chimnies which have been deſcribed we found iron rods affixed, reaching between four and five feet above the top of the chimney, pointed at the upper end, and tapering about ten inches to that point. Each rod or bar was nearly ſquare, meaſuring, upon a mean, about half an inch one way, and four-tenths of an inch the other, with the angles juſt rounded off. Theſe conductors were continued down the building by a ſucceſſion of ſimilar bars of iron, in general from ſix to eight feet long, joined to one another by two hooks and nuts (ſee fig. 12.); that is, the correſponding ends of each bar being formed into a hook bent at right-angles, the hook of the uppermoſt went into a hole of the lowermoſt, where it was faſtened with a nut, and the hook of the lowermoſt went into a ſimilar hole of the bar above, where it was fixed in the ſame manner; the length of each of theſe joints, from nut to nut, was about two inches.

Though there were eight of theſe conductors reaching above the chimnies, yet they had only four terminations below. For the conductors to the two chimnies D and E (fig. 1. and 2.) being continued toward each other along the roof, united in the valley over the lead gutter there (at L in the 1ſt, 2d, and 3d figures), and from that point only one conductor was continued down the valley toward the ground. In like manner the two conductors from the chimnies A and C (fig. 1.) united in the valley of the roof between them, and were carried down toward the ground as a ſingle rod. All the three conductors from the chimnies F, G, and H, ſucceſſively joined together (ſee M, N, fig. 1.), and only a ſingle rod was continued from them down the lower part of the building. Laſtly, the conductor from the chimney B (fig. 1.) went down ſingle all the way, without having formed a junction with any other.

As the conductors, therefore, in their paſſage down the building, were thus reduced to four, we are now to ſhew their four terminations. And, firſt, that from the chimney B, being the ſimpleſt, was carried down the weſtern ſide of the weſt flank, till it came very near the ground, when it entered a ſmall channel of brick-work, through which it was continued under the pavement into a narrow bricked drain, leading

through

through the wall of a privy (at O, fig. 1.) into which the drain diſcharges itſelf. The conductor having paſſed, in the drain, through the hole in the privy wall, was continued about three feet into the open ſpace under the ſeat of the privy, where it terminated in air, none of the ſolid work being nearer its end than ſix inches. As this drain is conſtructed to receive the foul water from the yard, and one of the water-cocks is near, ſome moiſture will moſt commonly be found in it; but the ſtones ſlope ſo rapidly at the termination of the drain in the privy, that any water which runs thither muſt be immediately carried off. This conductor, as well as all the others, was kept in its place near the wall in its paſſage down, by ring-ſtaples driven into the wall.

The ſecond termination we ſhall deſcribe is that belonging to the chimnies F, G, H (fig. 1.). The conductor from the chimney F in paſſing down the roof was joined by that from the chimney G (at M, fig. 1.) and afterwards by the rod from the chimney H (at N, fig. 1.); thence the iron was continued down till it came near the bottom of the wall, where it was turned off along the pavement toward a ſink (fig. 1. Q) not quite two feet diſtant, through the ſide of which it was carried, and projecting four inches into the open ſpace of the ſink, there terminated in air. The ſink is built of brick, one foot nine inches deep, and two feet and a half ſquare within; and into its ſouth ſide is fitted an iron grate, of the ſame length as the ſide externally, and about ſeven inches deep, the lower part of which lies on the bare ſoil. Through the middle of this grate the conductor paſſes, reſting in contact with one of the bars. From its termination to the bottom of the ſink is not leſs than twelve inches; and the bottom, which is of brick, ſlopes ſo much, that water can never lie upon it, there being a large drain on the further ſide, which leads off from the bottom of the ſink.

The third termination to be inveſtigated is that formed by the conductors from the chimnies A and C (fig. 1.). Theſe, after joining in the valley which lies between them, ran down, as a ſingle rod, over the lead covering the valley, paſſed through a hole in that lead, where it projects over as a ſpout, and deſcended in the angle formed by the interſection of the weſt flank with the front of the center range (S, fig. 1.). Being arrived within eight inches of the ground, it entered a narrow channel of brick-work, through which it was conveyed into a ſmall cloſe drain or gutter, where it terminated, with a hooked end, in contact with one of the ſide bricks. It touched nothing ſolid, therefore, in its courſe under the ground, but brick-work. The ſmall drain in which it terminated was ſo placed as not to receive much moiſture; and this drain led into the ſide of a grated ſink (U, fig. 1.), at the bottom of which the great drain of the fore-court begins.

Of the ſeveral conductors that have been hitherto conſidered, the different parts of the building to which they were affixed, and their reſpective terminations, very accurate drawings were made on the ſpot; but as theſe conductors were more diſtant from the ſtricken end of the houſe than thoſe which remain to be deſcribed, and alſo ſhewed no marks of having been affected by the ſtorm, we thought it unneceſſary to enter into a more circumſtantial detail regarding them; eſpecially as, if any further particulars ſhould appear hereafter to be of conſequence, it will at all times be eaſy to refer to the original drawings and notes.

We proceed now to examine the fourth termination, by which the conductors from the chimnies D and E (ſee fig. 1.

and 2.), being thoſe neareſt the ſtricken corner, were carried under the ground. The conductor of the chimney D, from its upper point to its final termination, conſiſted of ten bars, into the ſixth of which the conductor coming from the chimney E was faſtened by its fourth bar, reckoning from the top (at L, fig. 1. and 2.). This junction was made by a hook at the lower end of that fourth bar of the conductor from the chimney E, which hook was received into a hole of the abovementioned ſixth bar, and fixed there by a nut underneath. Here was, therefore, only one hook and nut, inſtead of two as in the common joints. Alſo at the top of this ſixth bar of the conductor from the chimney E, where it united with the fifth bar, only one hook and nut were employed to form the junction, the other hook appearing never to have been put into its correſponding hole. In this ſame ſixth bar, above the hole into which the conductor from the chimney E was inſerted, we found four other ſpare holes, which were left quite empty.

Tracing the conductor downward from this point of union, we found it deſcend over the lead of the valley, to the ſurface of which it gradually approached, till at a hole made on purpoſe (*m*, fig. 2.) it paſſed through the lead, whence it was continued down the angle formed by the interſection of the eaſt flank with the front of the center range (T, fig. 1. 2. and 5.). It no where touched the wall of the building, but was kept in its place by ring-ſtaples (*p*, *p*, fig. 3.). Being arrived within two or three inches of the ground, it entered into a channel of brick, encloſed on all ſides (at *e*, fig. 5.), in which it was continued down to the arch of the great drain of the fore-court (*x*, fig. 5.); here, having paſſed through a hole in the haunch of the arch (*y*, fig. 5. and 7.), it was bent off from the houſe through the middle of the drain, and ultimately terminated in contact with the bricks at the bottom of it (at z, fig. 5. and 7.). This conductor, therefore, in its paſſage downward, did not communicate, till it reached the bottom of the drain, with any thing better able to carry off electricity than maſonry or timber; for the iron-ſtaples faſtening it to the wall, and the lead lining the valley, were themſelves in contact with ſuch ſubſtances only.

As this drain, then, is the real termination of the conductor, it muſt now be more attentively conſidered. It begins at the weſtern ſink of the fore-court U (fig. 1.); thence it is continued (V, V, fig. 1.) with a proper declivity to the eaſtern ſink W (fig. 1. 5. and 6.); it then runs under the eaſt flank of the houſe (V2, fig. 1. and 5.), and ends beyond it in the ſide of the ceſs-pool P (fig. 1.). From the grating on the ſink U to that on the ſink W (fig. 1.) is 89 feet, and thence to the ceſs-pool P near 69 feet; the breadth of the drain at bottom (z, fig. 7.) is 14 inches; its height to the ſpring of the arch (fig. 7.) 16 inches, and to the crown of the arch (x, fig. 5. and 7.) 23 inches. When we ſaw it, the moiſt filth, or ſludge, at bottom (z, fig. 7.) was two or three inches deep; but when the court is overflowed, as the two grates (at U and W, fig. 1.) are laid on purpoſe to receive the ſuperfluous moiſture, there muſt be ſome run of water through it. We eſtimated the fall of the drain, from the eaſtern ſink W (fig. 1.) to its termination in the ceſs-pool P, at two feet. The ceſs-pool itſelf reſembles a well, walled round in the inſide, and has foul water ſtagnating at the bottom, which cannot riſe above a certain height on account of a large drain, leading from it into the great reſervoir (at R, fig. 1.), out of which the foul water is ultimately pumped. When we examined this ceſs-pool, the water in it ſtood even with the bottom of that great drain, conſequently was almoſt as high as it could be, unleſs the drains ſhould at any time be flooded; and upon meaſuring the diſtance from the bottom of the drain coming from the fore-court (V2, fig. 1.) where it terminated in the ſide of the ceſs-pool, down to the ſurface of the water ſtagnating in the ceſs-pool, we found it 3½ feet. This interval, therefore, of three feet and a half muſt be paſſed through, to form a communication between the water in the drain, and that in the ceſs-pool. The drain is firmly built of brick and mortar (ſee the ſection of it, fig. 7.). To determine the nature of the ſoil in which it is laid, a hole was dug in the fore-court ſeven feet deep, where we found nothing but ſand, at this time pretty moiſt, with a few pebbles. There is reaſon to believe, however, from the ſoil of an adjacent declivity to the northward, that below the ſand, perhaps at the depth of 15 or 16 feet, a bed of clay would be found.

Againſt the eaſt flank, near the corner T (fig. 1.), there riſes a leaden pipe with a cock (O, fig. 2.), to which the water is conveyed from a raiſed ciſtern (ſee *r*, fig. 1.) in one of the detached offices of the back-court. A main of lead from the ciſtern, which is itſelf of that metal, after ſending out pipes to ſome other cocks, and paſſing through the cellars of the houſe, comes into the fore-court about four yards from the corner T (fig. 1.) and is carried over the drain at the diſtance of about a foot above its crown, and eight inches below the ſurface of the ground. Here it divides into two branches, one of which goes ſtraight to the cock at O (fig. 2.); and the other runs weſtward, to ſupply a ſimilar cock in the oppoſite corner. We meaſured the diſtance of theſe pipes and cocks from the conductors, and found that they came no where nearer than five feet and an half.

Such were the conductors that, in the month of June, 1777, ſeveral years after the Houſe of Induſtry had been built, were erected with the hope of guarding it from lightning. The iron of which they were formed had in that time acquired a coat of ruſt, ſuch as might be expected from four years expoſure to the air. On the 17th of June, 1781, after a ſhowery forenoon, a heavy cloud having riſen from the S. W. brought on a ſevere thunder-ſtorm, attended with ſuch heavy hail and rain, that the court before the houſe was overflowed. At length, about three in the afternoon, when this ſtorm had already laſted 15 or 20 minutes, a ſingle and very loud exploſion was heard, like the report of a cannon, which exceedingly terrified all the people in the houſe, and affected three of the paupers ſo much that they fainted. At the ſame time a great light was perceived, which ſeemed, as they expreſſed it, to come in at the windows, and ſtill more at the doors of the rooms, like a ſheet of fire. Within one or two minutes, the ſouth-eaſt corner of the eaſt flank of the building was obſerved to be on fire, the flame burſting out at the bottom of the hip (ſee *g* in fig. 3. 4. and 11.). By the briſk exertions of the people in the houſe, this fire was quickly extinguiſhed; and the court was ſo overflowed, that they procured ſufficient water for that purpoſe by means of a hole which they dug near the burning corner of the building. The ſtorm, and eſpecially the rain, continued ſome time after the ſtroke, but not with ſuch violence as before. At the moment of the exploſion it was nearly calm; but the wind had been ſouth-weſterly all day, and the ſky was obſerved to be clearing in that quarter about the time of the accident.

To come at the fire, in order to extinguiſh it, the lead had been rolled off the bottom of the hip, and ſome bricks thrown down, all of which were replaced when we arrived at Heckingham;

ham; but as the men, who had gone up to the corner of the houſe on the firſt appearance of the fire, ſeemed to recollect very well the ſtate in which they had found the lead and tiles at that moment, they were deſired to put every thing in the ſame ſtate to the beſt of their memory. With this view they turned back the lead at the bottom of the hip on its ſouth ſide, ſo that the ſouth-weſt face of the hip-pole might be ſeen, and threw down a few tiles, after ſetting one on edge againſt the hip-rafter. The lightning then, if ſuch evidence be admitted, had raiſed up that corner of the lead to the breadth of about ſix inches at the bottom, and diſplaced ſome tiles. An effect of this kind upon the lead, is one of the commoneſt facts obſerved in buildings that have been ſtruck by lightning. It ſo happened, that the piece of lead which we found on the bottom of the hip at Heckingham, had upon it ſeveral impreſſions or pits; concerning which various opinions were entertained, till an experiment, made ſince our return to town, ſeems to have put it beyond doubt, that they are nothing but marks of large ſhot, ſuch as might have been produced by firing, with a large fowling-piece, at a bird ſitting on the corner of the houſe. All the people who aſſiſted in extinguiſhing the fire agreed, that on the eaſtern ſide of the hip, the lead remained, after the ſtroke, in its uſual ſituation.

On removing entirely the lower part of the lead, no kind of damage was ſeen on the wood of the hip-pole, except that near the lower end it was ſlightly ſcorched in one place, apparently by the flame which had burſt forth from below; the ſpike-nail which had faſtened the lead to it appeared perfectly ſound, and even the hole made by that nail in the wood was neither burnt nor ſplintered. This hip-pole was ſupported, at its proper diſtance from the hip-rafter, by an iron-ſtrap, or holdfaſt, which was driven into the timber making the tie of the angle, through the bevelled end of the hip-rafter, juſt without the part where the tenon of the latter is received into the mortiſe of the former (*a*, fig. 8.). Here it was that the fire ſeemed to have begun, though neither the holdfaſt itſelf, nor the hip-pole reſting upon it, ſhewed any ſigns of the lightning. From the place into which this holdfaſt was driven (*a*, fig. 8.) to the outer end of the angle-tie (*b*, fig. 8.) there was a conſiderable loſs of ſubſtance, occaſioning a large hole; but the ſides of the hole within were ſo ſmooth, and ſo little charred, ſhewing plainly the grain of the wood, that it was ſcarcely poſſible to ſuppoſe the whole had been *burnt* out; we conjectured, therefore, that a large ſplinter had been forced off by the lightning at this place, and, in the ſame moment, the tenon of the hip-rafter ſet on fire where it enters the mortiſe. Indeed, unleſs ſome opening had been made by forcing out ſuch a piece, it does not appear how the fire could have burnt, for want of air, in a part that is always ſo cloſely joined by builders: and yet, in this confined place, the tenon of the hip-rafter was ſo far conſumed, that a ruler could be thruſt in, almoſt to the further extremity of the mortiſe. From this ſpot the flame ſeems to have iſſued out eaſtward, between the tie of the angle and the wall-plate (*c*, *c*, fig. 8.) ſcorching all the timbers it could reach, and ſetting fire to the laths; but the miſchief it had done was very trifling (ſee fig. 8.).

Juſt beneath the abovementioned hole at the end of the angle-tie (*b*, fig. 8), is the extremity of the wall-plate which lies upon the eaſtern wall of the eaſt flank (*d*, fig. 8.). The end of this wall-plate was rent in a remarkable manner (*e*, *e*, *e*, *e*, fig. 8.), and ſeveral of the fiſſures were continued ſome way upon the ſides (*d* and *f*, fig. 8.). Though the other timbers we have mentioned are of fir, the wall-plate is of ſolid oak; and the violence done to its extremity was ſuch, that we could not doubt but it had been occaſioned by the lightning.

Under this end of the wall-plate there was a crack in the ſouth face of the corner (*m*, fig. 8.), which went down four courſes of bricks, and then terminated abruptly (*m*, fig. 3.). The three external courſes of bricks above this crack were new, and projected out much farther than the others, to form the cornice of the wall. Whether the bricks of the old cornice had been damaged or thrown down by the ſtroke, we could not learn with certainty; but the general report among the people we conſulted was, that they had not, and were only taken down to extinguiſh the fire: this opinion ſeemed probable from the want of marks on the hip-pole which projected out with the cornice, and the appearance of ſuch ſtrong effects of lightning on the wall-plate which lay within any part of the projection; whence it might be concluded, that the lightning paſſed within the cornice, and no where through it. Between the bottom of the wall-plate, and the top of the crack where it appeared to begin at the foot of the cornice (*m*, fig. 3. and 8.), lay two inner courſes of bricks (*o* and *p*, fig. 8.) covered by the cornice. Some damage had evidently been done to the bricks in this part, though we could not diſtinctly trace the progreſs of the lightning through them.

Beneath the eaſt edge of the wall-plate, and ſeparated from it in like manner by two courſes of bricks, a ſimilar crack deſcended from the bottom of the cornice (*l*, fig. 4. and 8.) on the eaſt face of the corner, and went through ten courſes of bricks till it reached the top of the wall that ſupported the ſtable. Here the three bricks next the houſe, it is ſaid, were ſhivered into pieces as ſmall as nuts, but not thrown off (*c*, fig. 10. and 11.). The cracks in the bricks on both faces of the corner remained, having only been filled up with mortar; but new bricks were put in the place of the three that had been broken on the wall. All the workmen we ſaw agreed in opinion, that no iron cramps, or other metal, had been uſed in the brick-work.

Beginning from theſe three ſhivered bricks on the top of the wall, three courſes of pantiles on the roof of the ſtable, in the direction downward, were in great meaſure broken or diſplaced, except about two feet of the lower end of the courſes, near the eaves, where the tiles remained untouched (ſee *c*, *m*, *g*, *q*, fig. 10. and *c*, *q*, fig. 11.). All theſe pantiles reſted upon laths, which were faſtened to the rafters of the roof by iron nails about eleven inches aſunder. Within the ſtable, and almoſt underneath the ſpot where the damage to the pantiles ceaſed, a ſaddle hung, at the time of the accident, by a nail driven into the wall at that weſt end of the ſtable, which was alſo the eaſtern wall of the eaſt flank of the houſe (*n*, fig. 10.). As this ſaddle, being much torn by the lightning, ſeems to have been the ſtep by which it paſſed through the ſtable, the reſpective ſituations of all their parts ſhall be minutely deſcribed.

The ſtable in its inſide is 25 feet long (from *r* to *s*, fig. 9.), 13 feet broad (from *t* to *u*, fig. 9.), 15½ feet high on its ſouth ſide (from *w* to *x*, fig. 10.), and 7½ feet on the north (from *y* to *z*, fig. 10.). At the weſt end is a ſtall for one horſe (*s*, fig. 9.). Near the middle of the north wall is a drain (*y*, fig. 9. and 10.), which terminates juſt without the wall in the garden (*k*, fig. 9. and 10.). Againſt the weſt end of the ſtable, a ſhelf (*e*, *f*, fig. 10.) was ſupported by two nails underneath (*f*, fig. 10.). Seven inches and a half below this ſhelf was a nail, on which the ſaddle hung by one of its ſtirrups (*n*, fig. 10.). The breadth of the ſhelf was near one foot and an half; its length (from *e* to *f*, fig. 10.) two feet five inches; and the pantiles ſeem to have been diſplaced a little farther down on the roof (*g*, fig. 10.) than the line correſponding perpendicularly with the north end of the ſhelf (*h*, fig. 10.). Neither the ſhelf, nor the nails ſupporting it, which were both near its north end, ſhewed any ſigns of injury; whence it may be conjectured, that if the lightning took its courſe this way, it paſſed obliquely between the ſaddle and the roof, ſo as to miſs the edge of the ſhelf, leaving it to the ſouthward. The upper part of the ſouth ſide of the ſtable was boarded off from the reſt, to form a hay-chamber, which occupied ſo large a portion of the roof (from *x* to *m*, fig. 10.), that the boards of the perpendicular partition (at *o*, fig. 10.) came within ten inches of the nail on which the ſaddle hung. Theſe boards were faſtened to the uprights of the partition, all the way down from the roof, by nails about ſix inches aſunder, conſequently ſome of thoſe nails muſt have been within ten inches of the ſtirrup-iron as it hung on the nail in the wall (*o* and *n*, fig. 10.). No tokens of the lightning could be diſcovered on thoſe boards, or the nails faſtening them; we could not, therefore, be certain, whether any part of it had paſſed that way. The nail which ſupported the ſaddle was equally free from marks; but one of the ſtirrup-leathers was much torn and burnt, and a large piece of the leather was ſtripped off the ſeat of the ſaddle, beſides other damage done to it in that part. One of the ſtirrup-irons, likewiſe, exhibits ſome appearances of fuſion on the arch through which the ſtirrup-leather paſſes. This iron, as well as the ſtirrup-leather, being the only damaged parts of the ſaddle that remained, we have brought for your inſpection.

It muſt be evident that we derived the knowledge of moſt of theſe circumſtances relative to the effects of the lightning upon and within the ſtable from information, the damages having been repaired before our arrival. As the workmen preſent, however, agreed pretty well in their teſtimony, and it was corroborated by every thing that appeared, we deſired them to replace all the parts as they were left by the accident, and thence made the deſcriptions and drawings. We gave directions that a man, accuſtomed to the ſtable, ſhould hang up the ſaddle there in the uſual manner, and then aſcertained the following meaſures:

	Ft.	In.
From the nails ſupporting the ſhelf in the ſtable (at *f*, fig. 10.) to the neareſt nails of the pantile laths of the roof (about *b*, fig. 10.) - - -	1	3
From the ſouth end of the ſhelf (*e*, fig. 10.) to the roof over it (*m*, fig. 10.) - -	2	1
N. B. The ſouth end of the ſhelf was fixed to the partition-boards of the hay-chamber (*e*, fig. 10), and the two nails under its north ſide (*f*, fig. 10.) were 5½ inches apart.		
From the neareſt of the nails ſupporting the north end of the ſhelf (*f*, fig. 10.) to the nail on which the ſaddle hung (*n*, fig. 10.) - -	0	8
Length of the ſtirrup-iron below the nail (*n*, fig. 10.) - - - - -	0	3½
Length of the ſtirrup-leather, from the ſtirrup-iron to the ſeat of the ſaddle - -	1	9
Breadth of the ſeat of the ſaddle - about	0	8
Diſtance from the lower ſide of the ſeat, as the ſaddle hung, to the bottom of the loweſt ſtirrup-iron (*p*, fig. 10.) - - - - -	2	0
Diſtance from the loweſt ſtirrup-iron (*p*, fig. 10.) to the floor of the ſtable (near *d*, fig. 10. and 9.) - - - - about	3	6

As the ſaddle was thus placed by recollection, the girths reached from it to the ground (*d*, fig. 10. and 9.); but neither theſe girths, nor any other part of the ſaddle, except one ſtirrup-iron, one ſtirrup-leather, and the ſeat, were ſaid to have been damaged by the accident.

From the quantity of rain which fell in the thunder-ſtorm, the ſtable was overflowed with water, which gradually ſunk into the drain (at *y*, fig. 9. and 10.). The leather ſtripped off the ſeat of the ſaddle was found in the ſtable near this drain; whether thrown there originally, or carried by the water, is uncertain. From the point of the floor immediately under the ſaddle, to the neareſt part of the drain, was about 12½ feet; the width of the drain (*y*, fig. 9.) 14 inches; its length through the wall to the edge of the hole or ſink into which it diſcharges itſelf 18 inches, and the depth of the ſink from the bottom of the drain about one foot and an half. As this ſink was merely a hole, without any drain leading *from* it, and was bricked at the ſides, the water could not paſs off by the drain of the ſtable any faſter than it could ſoak through the looſe ſoil at the bottom of the ſink. And it is evident, from this conſtruction, that the earth under the ſink will uſually be ſome of the wetteſt near the building, and be impregnated with ſalts from the ſtale of the horſes.

Except the marks which have been already deſcribed, we could not find on any part of the ſtable, either within or without, the leaſt veſtige of the lightning. We particularly examined the lead *flaſhing* on the top of the roof (from *c* to *d*, fig. 11.), and the hay-chamber immediately under the three broken bricks and the diſplaced pantiles, but in vain. There was a hook fixed in the wall, 15 inches below the nail on which the ſaddle hung, and ſo exactly underneath, that the ſtirrup-leather may be ſuppoſed to have covered it; but this alſo appeared to be perfectly untouched. After making every poſſible inquiry, we could not determine by evidence, whether the ſtirrup-leather which is ſo ſinged and torn was the upper or the lower one at the time of the accident. Much leſs could we get information of the reſpective poſitions of the two ſtirrup-irons. But, whatever their ſituation may have been, as ſo few ſteps were to be traced, it would ſeem that the lightning muſt have jumped over at leaſt one long interval in its paſſage through the ſtable.

About ſeven feet from the ſtricken corner of the building, and almoſt two feet from the neareſt part of the roof of the ſtable, is a window (A, fig. 4. 10. and 11.) being the ſouthernmoſt of the upper range on the eaſt face of the flank. It has thirty ſmall panes of glaſs, ſet in lead. We were informed, that about half of theſe had been broken by the accident, chiefly on the ſide next the corner; and that the fiſſures ran in general horizontally, nearly parallel. Very little, if any, of the glaſs was forced out. As we could not diſcover any trace of the lightning directly toward this window, a ſuſpicion aroſe, whether it might not have been broken rather by the general concuſſion than by any immediate ſtroke.

Having examined all the marks that appeared between the bottom of the ſtricken hip and the ground, our next inquiries were directed to the top of the hip (*f*, fig. 2. 3. 4. and 11.). Here the upper plate of lead (*e*, fig. 4.) which ſerved as a capping to the junction of the hip with the ridge of the roof, being taken off, we found, on its under ſurface, three diſtinct marks of fuſion; and on the upper ſurface of the ſheet of lead which it covered three correſponding marks, ſo exactly ſimilar, that the two ſurfaces of lead ſeem to have touched one another in a melted ſtate. Theſe fuſed ſpots are juſt in the bend of the lead, anſwering to the obtuſe angle formed between the hip and the roof (*f*, fig. 4.). We obtained leave to bring away both the pieces of lead, and now preſent them for your inſpection. The workmen who examined the timber underneath reported, that it was not damaged; nor were any other ſigns of the lightning perceived in the whole length of this ſtrip of lead from the top to the bottom of the hip. In the pieces of lead which exhibit the melted ſpots on one ſurface, the other ſurface is perfectly clear of all marks, though the latter was, in the uppermoſt plate of lead, that which had lain expoſed to the clouds. Neither of them is melted to any depth into the ſubſtance of the metal.

As both extremities of the hip, therefore, were, in ſome degree at leaſt, affected by the lightning, we proceeded to aſcertain their diſtances from the neareſt conductor, which was that affixed to the chimney E (fig. 2. and 4.). Having determined the neceſſary meaſures, and calculated the hypotenuſe, the diſtance from the point of the conductor to the beginning of the lead on the top of the hip (*e*, fig. 4.) came out 42 feet and a quarter; thence to the bend where we found the marks of fuſion (from *e* to *f*, fig. 4.) was five or ſix inches more; and as the hip meaſured about 27 feet in length, the diſtance from the conductor to the bottom of the hip (*g*, fig. 4.) may be called 69 feet. From the top or bottom of the hip, to the neareſt part of the conductor as it ran downward, the diſtances were not a foot leſs than theſe meaſures. We then took down the uppermoſt rod of the conductor, and carefully examined it, eſpecially at the point, and at the hook and ſcrews by which it had been joined to the ſecond rod; but could no where diſcover the leaſt mark of fuſion or other injury. At the bottom of this conductor, however, where, having joined that from the chimney

D, it terminated in the drain (ſee the general plan, fig. 1. and 2, fig. 5.), a ſmall bright ſpot appeared on one of the angles. As ſome ſuſpicions were entertained, whether this mark might not have been occaſioned by the lightning, we cut off the end of the rod, and have brought it hither for public examination.

Where this conductor entered its channel at the corner of the court (ſee T, fig. 1. and *e*, fig. 2. and 5.), the ground is raiſed ſo much above the grate of the ſink (W, fig. 1. and 5.) that, though the court was overflowed, it is not probable, the water could have riſen high enough to run into the channel (at *e*, fig. 5.), and ſo communicate with the conductor before it reached the drain.

Cloſe to the chimney E, a dinner-bell hung in a common frame (*q*, fig. 2. and 4.). Three different perſons went up to examine this bell; but could not diſcover upon it any where the leaſt veſtige of the lightning.

Such are the facts we were able to collect from an aſſiduous examination of the Poor-houſe at Heckingham, and of thoſe witneſſes in the neighbourhood who knew any thing of the accident. We have ſtated the appearances as they preſented themſelves to us, with all the minuteneſs that could be preſerved without too much crowding the narrative, and independently of any opinions. Whether the earth or the clouds were poſitive at the time; whether the top or bottom of the hip was firſt affected by the ſtroke; whether all the lightning took its courſe through the hip, or part went that way, and part through the conductor; and how far the conductors were properly conſtructed, or adequately terminated; are queſtions which will naturally ſuggeſt themſelves to your conſideration.

It may be proper, however, to add the two following pieces of information.

One of the cripples in the Houſe of Induſtry, a middle-aged woman, aſſured us, that at the time of the accident, as ſhe was looking from the door of the hall (which is in the center of the front facing the ſouth), ſhe ſaw three balls of fire dart down; that one fell exactly oppoſite her; a ſecond ſeemed to ſtrike the corner of the houſe; and the other deſcended in the direction of a door in the eaſtern flank, which was not far out of the perpendicular line of the chimney E (ſee the general plan, fig. 1.). If any credit could be given to the teſtimony of ſuch a perſon in a matter like this, it would incline us to believe, that the exploſion was made in three ſtreams, of which one paſſed through the conductor of the chimney E, and another through the damaged corner of the houſe; whilſt the third fell on the ground, or, as the woman deſcribed it, on the great gate of the fore court near the lodges (ſee the general plan, fig. 1.). We examined the gate and lodges, with the adjacent parts, but could no where diſcover any marks of injury; nor could we learn that any place in the neighbourhood had been ſtruck, or that any perſon, except this woman, pretended to have ſeen the courſe of the lightning.

In our return to town, through Norwich, we ſaw an ingenious gentleman of that city, who ſays, that he found the clouds negative there on the day of the accident at Heckingham. The two places are diſtant about eleven miles by the road.

It would be unpardonable to conclude this Report, without expreſſing our obligations to the Directors and Guardians of the Houſe of Induſtry at large, and to the neighbouring

Gentlemen in particular, for the liberal manner in which they ſeconded our endeavours to execute the commiſſion with which you had charged us. By their kind aſſiſtance proper workmen were provided; and every accommodation afforded us, that could contribute to the inveſtigation of this remarkable accident.

We have the honour to be,

GENTLEMEN,

Your moſt obedient humble ſervants,

C. Blagden.
Edw. Nairne.

London, Feb. 7, 1782.

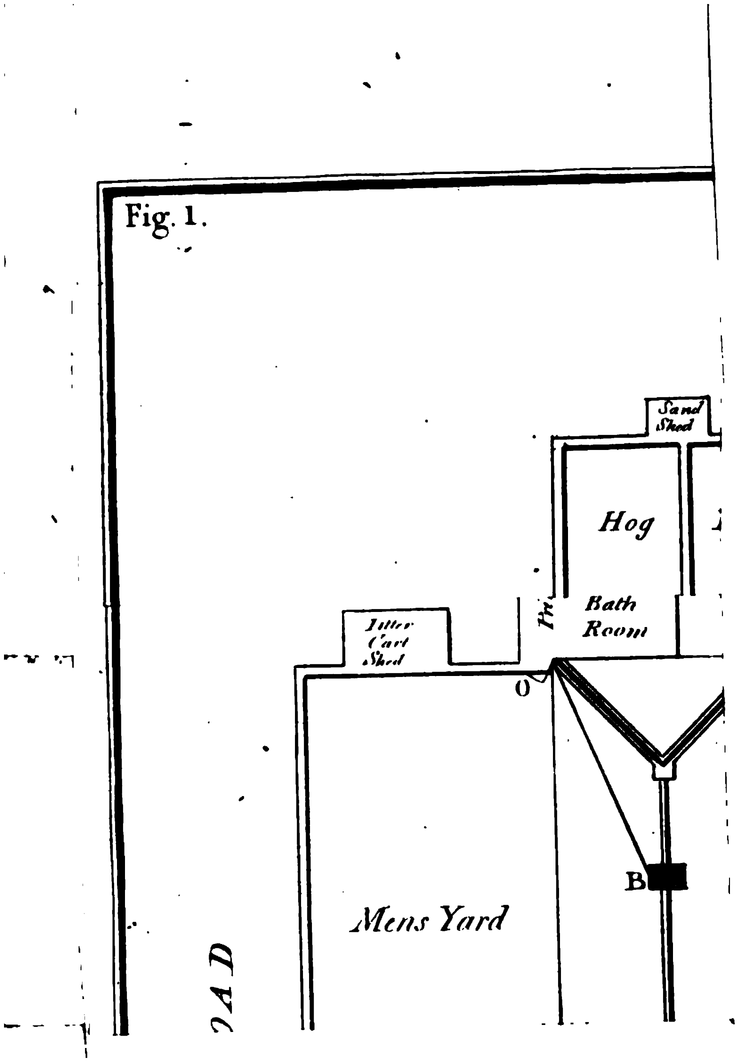
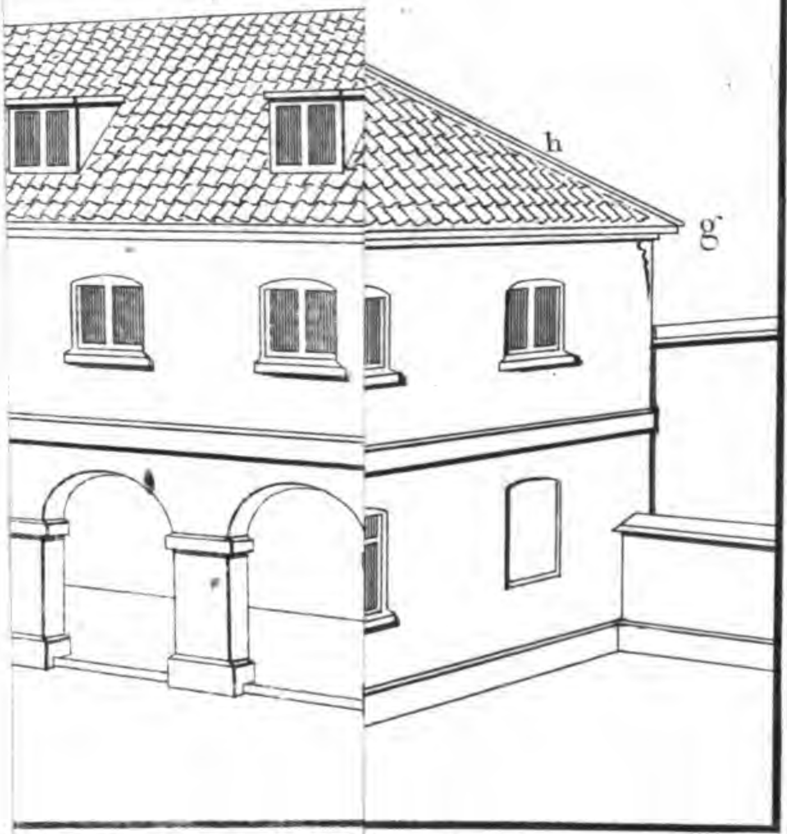


Fig. 1.

EAST WING and E

co Conductors that to.



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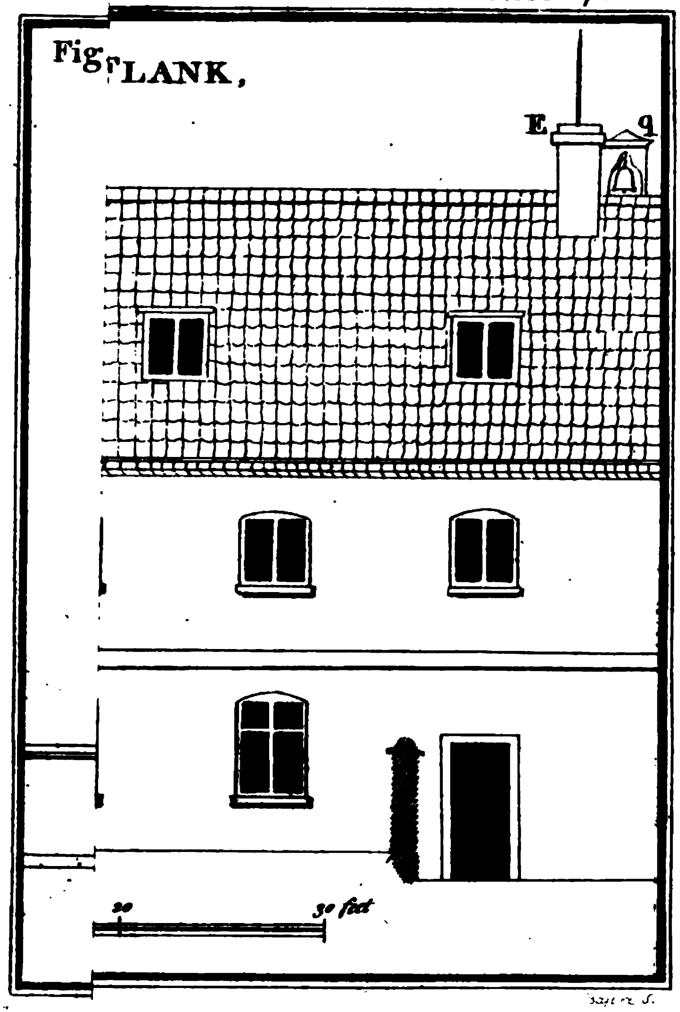

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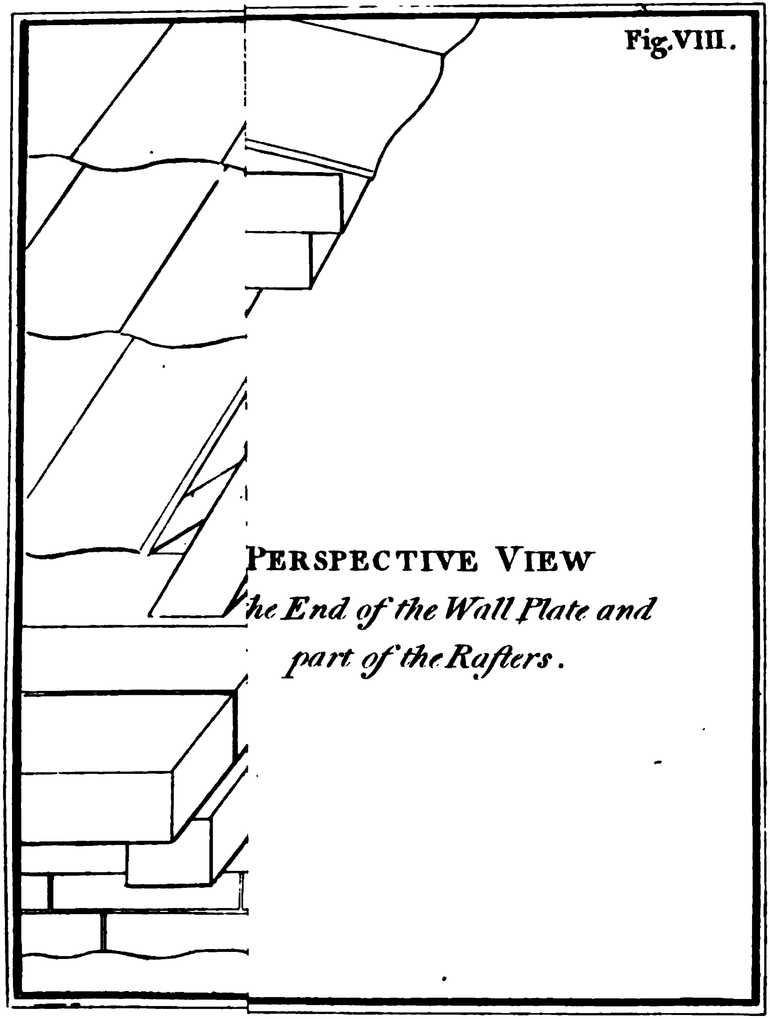
Fig. VIII.



PERSPECTIVE VIEW

he End of the Wall Plate and part of the Rafters.

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Fig. X.

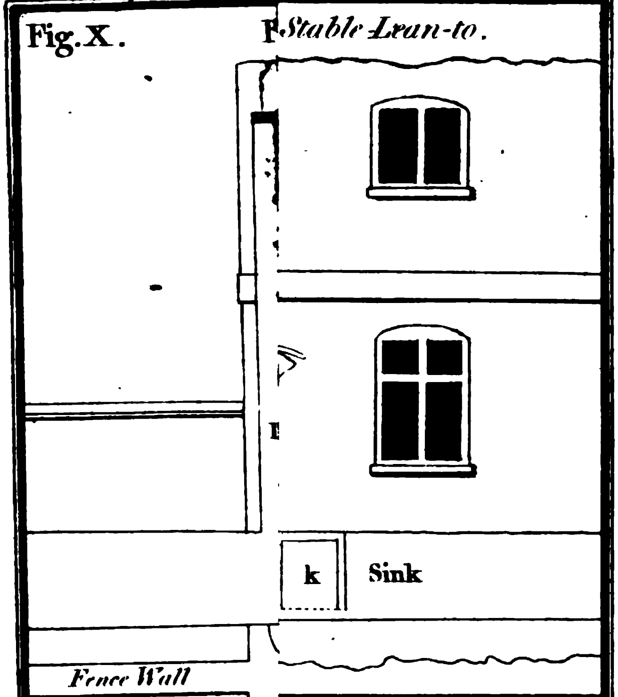
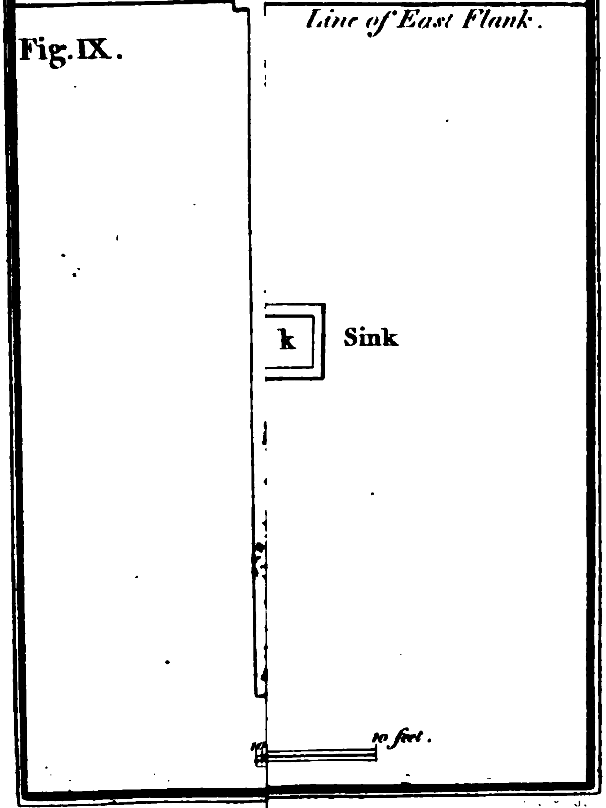


Fig. IX.



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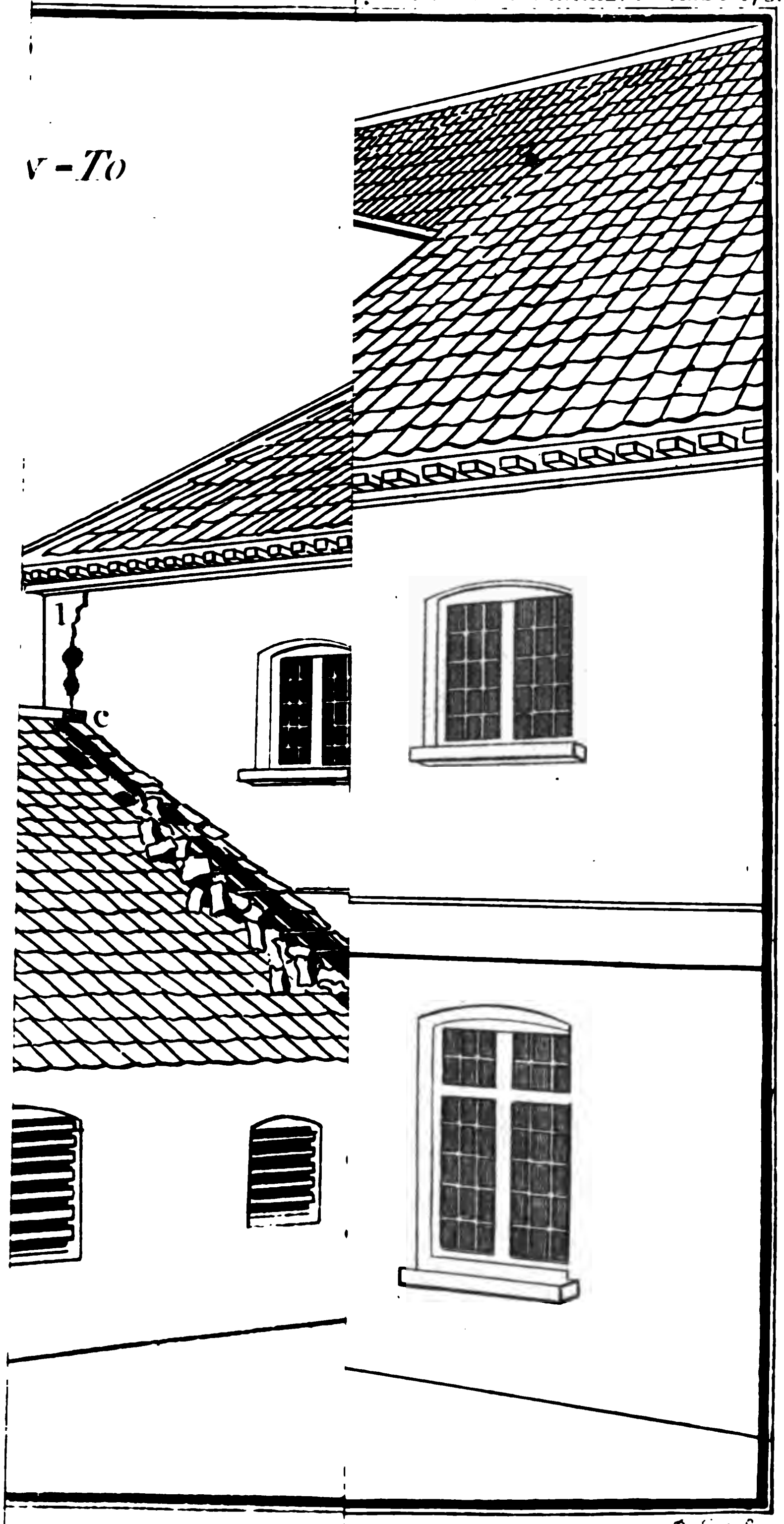

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XXIII. *Account of the Organ of Hearing in Fiſh.*

By John Hunter, *Eſq. F. R. S.*

Read Nov. 14, 1782.

NATURAL hiſtory has ever been conſidered as worthy the attention of the curious philoſopher, and therefore has in all ages kept pace with the other branches of knowledge; and as both arts and ſciences have, of late years, been cultivated to a degree, perhaps, beyond what was ever known before, we find alſo, that natural hiſtory has not been neglected; all Europe appears to be awake to it. In this iſland it has been purſued with more philoſophic ardour, than what was ever known in any country. It has become the ſtudy of men of independent fortunes, who not only ſpend their fortunes in the cultivation of this ſcience, but have riſqued their health and lives in purſuit of it, ſearching unknown regions to improve mankind, ſettling correſpondences every where, ſo as to bring in its materials into this country, in order to make it the ſchool of natural hiſtory. It is no wonder, then, that a ſpirit of inquiry is diffuſed through almoſt all ranks of men; and that though many cannot purſue it themſelves, yet they are eager to know what is already known, chuſing at leaſt to benefit by the induſtry of others.

Theſe reflections have induced me to trouble this learned Society with a ſhort account of the Organ of Hearing in

Fiſh, it being ſtill a ſubject of great diſpute, whether fiſh hear or not.

Some time between the years 1750 and 1760, I obſerved the organ of hearing in fiſh; and from that time to this, I only conſidered it as a link in the chain of the varieties in this ſenſe in different animals, in which there is a regular progreſſion, *viz.* from the moſt perfect animals down to the moſt imperfect poſſeſſed of this organ *.

As I do not intend to give, in this paper, a full account of this organ in any one fiſh, or of the varieties in different fiſh, but only of the organ in general; thoſe who may chuſe to purſue this part only of the animal œconomy may think it deficient in the deſcriptive parts. If it was a difficult taſk to expoſe this organ in fiſh, I ſhould perhaps be led to be more full in my deſcription of it, but there is nothing more eaſy than the expoſure of this organ in this animal in general.

As this paper is to be confined to this order of animals, I may be allowed juſt to obſerve here, that the claſs called ſepia has this organ alſo, but ſomewhat differently conſtructed from what it is in the fiſh.

The organs of hearing in this latter order of animals are placed on the ſides of the ſkull, or that cavity which contains the brain; but the ſkull itſelf makes no part of the organ, as it does in the quadruped and the bird. In ſome fiſh this organ is wholly ſurrounded by the parts compoſing this cavity, which in many is cartilaginous, the ſkeleton of theſe fiſh being like thoſe of the ray kind; in others alſo, as in cod, ſalmon, &c. whoſe ſkeleton is bone, yet this part is cartilaginous.

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* Preparations to illuſtrate theſe facts have been ever ſince ſhewn in my collection to the curious both of this country and foreigners: when in ſhewing whatever was new, or ſuppoſed to be new, the ears of fiſh were always conſidered by me as one important article.

In ſome fiſh this organ is in part within the cavity of the ſkull, or that cavity which alſo contains the brain, as in the ſalmon, cod, &c. the cavity of the ſkull projecting laterally, and forming a cavity there.

The organ of hearing in fiſh appears to grow in ſize with the animal, for its ſize is nearly in the ſame proportion with the ſize of the animal, which is not the caſe with the quadruped, &c. the organs being in them nearly as large in the growing fœtus as in the adult.

It is much more ſimple in fiſh than in all thoſe orders of animals who may be reckoned ſuperior, ſuch as quadrupeds, birds, and amphibious animals, but there is a regular gradation from the firſt to fiſh.

It varies in different orders of fiſh; but in all it conſiſts of three curved tubes, all of which unite with one another; this union forms in ſome only a canal, as in the cod, ſalmon, ling, &c.; and in others, a pretty large cavity as in the ray kind. In the jack there is an oblong bag, or blind proceſs, which is an addition to thoſe canals, and which communicates with them at their union. In the cod, &c. this union of the three tubes ſtands upon an oval cavity, and in the jack there are two of thoſe cavities; theſe additional cavities in theſe fiſh appear to anſwer the ſame purpoſe with the cavity in the ray or cartilaginous fiſh, which is the union of the three canals.

The whole is compoſed of a kind of cartilaginous ſubſtance, very hard or firm in ſome parts, and which in ſome fiſh is cruſted over with a thin bony lamella, ſo as not to allow them to collapſe; for as the ſkull does not form any part of thoſe canals or cavities they muſt be compoſed of ſuch ſubſtance as is capable of keeping its form.

Each tube deſcribes more than a ſemi-circle. This reſembles in ſome reſpect what we find in moſt other animals, but differs in the parts being diſtinct from the ſkull *.

Two of the ſemi-circular canals are ſimilar to one another, may be called a pair, and are placed perpendicularly; the third is not ſo long; in ſome it is placed horizontally, uniting as it were the other two at their ends or terminations. In the ſkait it is ſomething different, being only united to one of the perpendiculars.

The two perpendiculars unite at one part in one canal, by one arm of each uniting, while the other two arms or horns have no connection with each other, and the arms of the horizontal unite with the other two arms of the perpendicular near the entrance into the common canal or cavity.

Near the union of thoſe canals into the common, they are ſwelled out into round bags, becoming there much larger.

In the ray kind they all terminate in one cavity, as has been obſerved; and in the cod they terminate in one canal, which in theſe fiſh is placed upon the additional cavity or cavities. In this cavity or cavities there is a bone or bones. In ſome there are two bones; as the jack has two cavities, we find in one of thoſe cavities two bones, and in the other only one; in the ray there is only a chalky ſubſtance †.

At this union of the two perpendiculars in ſome fiſh enters the external communication, or what may be called the external meatus. This is the caſe with all the ray kind, the external orifice of

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* The turtle and the crocodile have a ſtructure ſomewhat ſimilar to this; and the intention is the ſame, for their ſkulls make no part of the organ.

† This chalky ſubſtance is alſo found in the ears of amphibious animals.

of which is ſmall, and placed on the upper flat ſurface of the head; but it is not every genus or ſpecies of fiſh that has the external opening.

The nerves of the ear paſs outwards from the brain, and appear to terminate at once on the external ſurface of the ſwelling of the ſemi-circular tubes above deſcribed. They do not appear to paſs through thoſe tubes ſo as to get on the inſide, as is ſuppoſed to be the caſe in quadrupeds; I ſhould therefore very much ſuſpect, that the lining of thoſe tubes in the quadruped is not nerve, but a kind of internal perioſteum.

As it is evident that fiſh poſſeſs the organ of hearing, it becomes unneceſſary to make or relate any experiment made with live fiſh which only tends to prove this fact; but I will mention one experiment, to ſhew that ſounds affect them much, and is one of their guards, as it is in other animals. In the year 1762, when I was in Portugal, I obſerved in a nobleman's garden, near Liſbon, a ſmall fiſh-pond, full of different kinds of fiſh. Its bottom was level with the ground, and was made by forming a bank all round. There was a ſhrubbery cloſe to it. Whilſt I was laying on the bank, obſerving the fiſh ſwimming about, I deſired a gentleman, who was with me, to take a loaded gun, and go behind the ſhrubs and fire it. The reaſon for going behind the ſhrubs was, that there might not be the leaſt reflection of light. The inſtant the report was made, the fiſh appeared to be all of one mind, for they vaniſhed inſtantaneouſly into the mud at the bottom, raiſing as it were a cloud of mud. In about five minutes after they began to appear, till the whole came forth again.

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XXIV. *Account of a new Electrometer. By Mr.* Abraham Brook; *communicated by Sir* Joſeph Banks, *Bart. P. R. S.*

Read May 30, 1782.

AAAAN, fig. 1. repreſents the electrometer in full ſize and proportion, ſtanding on a table, or the like. The foot B is a ſquare piece of board, 9¼ inches each way, reſting on three pins C, C, *c*, ſeen at the under ſide of the foot. C, C, with the broad heads, are ſcrews to ſet the inſtrument upright withal. D is a ſolid piece of glaſs, which ſupports and inſulates the inſtrument from the place on which it ſtands. The arms G1 and *g*, with the ball F, turn round on the wire H (which is ſolid braſs, as may be alſo the arm *g*), and, when in uſe, are put near at a right angle with G2 and H, being turned to the off ſide ſo as to be as much as poſſible out of each other's atmoſpheres or the atmoſphere of a jar, battery, prime conductor, &c. The arms G1 and G2 are hollow tubes of copper, not ſo heavy as wires. The balls I1, I2, are made of copper, and hollow, ſo as to be as light as poſſible. K repreſents a kind of face or dial plate to the inſtrument with its index, which is carried once round by the motion of the arm G2 with its ball I2 moving through a quarter, or 90 degrees, of a circle; this motion is given to it by the repulſive power of the charge, &c. of electricity between the two balls I2 and L. The ends of the index from its center are of different lengths. The longeſt end reaches to a graduated circle, divided into 90 equal parts, anſwering to the 90°, which the arm G2 moves through. The ſhorteſt end reaches to a ſmaller circle, divided into 60 equal parts, anſwering to 60 grs. weight, or 60 diviſions, on the arm G1, with its ſliding weight *m*, each of which is equal to one grain, and the whole face is covered with a watch glaſs, to prevent the electricity from flying off at the points.

The top of the glaſs-ſupporter, or inſulator D, is cemented into a braſs cap M. This cap enters the ball L at bottom, and ſcrews into the upper part of the ball L at *a*. The top part of this cap M is tapered off to a cone about an inch and a half long or high. The lower end of the wire H has a hole made conically into it, ſo as to receive the upper part, or conical end, of the cap M, which permits all the upper part of the electrometer to turn round any way that may be neceſſary. The kind of ferrel O, with its baſe, is perforated for the lower end of the wire H to go through. The bent arm *b*, which ſupports the cup N, is ſcrewed into the baſe of the ferrel O, and turns freely round upon the wire H. The cup N is to receive the ball P of the arm, fig. 9. This arm ſhortens or lengthens, as may be wanted, by a wire ſliding into a tube. The end of the wire is ſlit, forming a ſpring in the tube to be ſteady. In this arm, fig. 9. is a kind of rule joint at *d*, that the arm may give way eaſily if wanted. The ſemi-circular end of the arm is a ſpring, and ſlips on to a ball from the prime-conductor, or the conductor itſelf (if they fit), jar, or battery. The ends of it are flat and broad, as repreſented in the drawing in miniature, of the electrometer at fig. 2. in the other drawing.

Fig. 2. to 11. ſhews the internal ſtructure of the electrometer.

Fig. 12. ſhews the part at *z* that ſcrews into the ball F, to ſupport the arm *g* with its ball *r*. This piece, which is made

hollow on the ſide next the wire H, ſo as to fit, and is ſcrewed in, ſo as to preſs againſt H, ſerves as a ſpring to keep the ball F ſteady, which ſlides up and down, as well as turns round, on H.

In order to make the diviſions of G1, fig. 1. exactly a grain each, firſt ſlide the weight *m* towards the ball F, fig. 1. till it is an exact counter-balance to the weight in F. At one end of the weight *m* let the diviſions begin; then ſuſpend any tolerable pair of ſcales, ſo that the bottom of one of them may reſt on the top of the ball *r*; then lay the ball I1 into the ſcale, and ſlide the weight *m* near to L1, and put as many grains into the other ſcale as will juſt raiſe the ball I1 in the ſcale; then mark the arm G1 at the ſame end of the weight *m*, and divide the ſpace between the two marks into as many parts as there are grains in the ſcale, which may be divided and ſub-divided into halves and quarters.

The arm G2 being repelled ſhews when the charge is increaſing, &c.; and I1 tells what ſuch a repulſive power is between two balls of the ſize of theſe in grains, according to the number the weight *m* reſts at when lifted up by the repulſive power of a charge. The longeſt end of the index K ſhews how many degrees of a circle G2 is repelled; and, by many trials, according to the number of grains, the arm G1 ſhews, when it is lifted up, and the weight *m* put at different places, ſuch reſpective numbers of grains may be marked on the leaſt graduated circle on the dial plate where the ſhorteſt end of the index points; ſo that when all the grains are thus marked on the dial plate, thus aſcertained by the arm G1, all theſe parts of the inſtrument, that is, the ball F with the arms G1 and *g* may be taken off, and the inſtrument is then graduated to be uſed without them; but I do not know how the grains can be ſo exactly marked and aſcertained as by theſe parts being on the inſtrument: nor do I mean to confine the number of grains or diviſions on G1; but, I think, my experience ſeems to tell me, that no glaſs to be *charged*, as we call it, with electricity, will bear a greater charge than that whoſe repulſive force, between two balls of this ſize, equals 60 grains weight, before it will be perforated or ſtruck through. Nay, I have not found many inſtances where it would ſtand 50 grains; and, I think, it is very hazardous to go more than 45 grains.

Thus, by knowing the quantity of coated ſurface, and the diameter of the balls, as I1 and *r*, I would ſay, ſo much or ſo much coated ſurface charged to ſo many or ſo many grains repulſion between two balls of ſuch or ſuch a ſize would melt a wire of this or that ſize, or do ſuch a thing, kill ſuch an animal, &c.; and if balls, wires, or arms of this ſize, are found too ſmall, larger may be made on the ſame plan.

In reſpect to the advantages of this electrometer above *thoſe now in uſe*, I do not, perhaps, know them all; and leſt my partiality may prejudice me in behalf of my own contrivance, would rather leave them to the judgement of others; my opinion however is, that all that I have ſeen or heard of are ſuch as ſpeak no intelligible language, and that this ſpeaks ſo as to be underſtood univerſally; for, unleſs the repulſive power of the charge of different glaſſes be very different, this electrometer, or any other electrometer, made after this manner, muſt, I ſhould think, ſpeak very nearly the ſame language, it being known how much coated ſurface there is, and the ſize of the balls; but if the ſize of the balls be not the ſame, the language the inſtrument ſpeaks will be very different. Although other Electrometers ſhewed a greater or leſſer charge or power, by an arm being repelled to a greater or leſſer diſtance, or by ſtriking differently at different diſtances, yet the power of the charge was not in any manner aſcertained; we could ſay, that the arm or index was repelled to ſuch or ſuch a number of degrees of a circle, or that it ſtruck to ſuch or ſuch a diſtance; but the repulſive power of a charge to repel the index ſo much, or ſo many degrees of a circle, or the ſtrength of the charge to ſtrike to ſuch a diſtance was not (that I know of) in any manner intelligibly aſcertained. This ſhews it by the weight that the repulſive power has to lift up in grains, &c.; which weight is to be proved by any tolerable pair of ſcales and weights; and I do not know any other method that has been yet tried to ſhew the different ſtrength of charges ſo good as that of repulſion.

All the neceſſary parts of the inſtrument being made of metal and glaſs that is pretty ſtout, I think, the electricity is leſs liable to eſcape than by wood, &c. I have tried reeds on account of their being light, and covered them with tin-foil, or gilded them to make them good conductors; but ſo frequently found inconveniencies from them by points riſing up, the celerity of moving, and the different weight of them at different times owing to moiſture, change of the weather, and the like, that I have laid them all aſide, and find my preſent inſtrument as free from theſe inconveniencies as I could expect; nor is it liable to be out of order, if proper care is taken of it.

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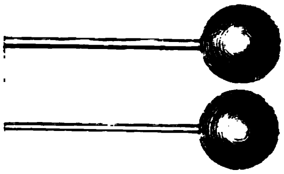

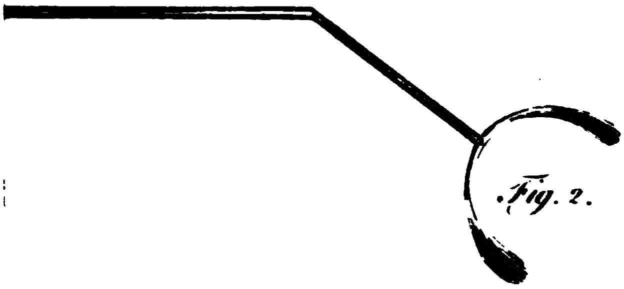

Fig. 2.

Fig. 1. represents the Electrometer in miniature. Fig. 2.
ns Arm to make a communication from a Battery, &c.
y the Semicircular (E) End being slipt on to a Ball of the
attery &c. and the Ball A being laid into the Cup A Fig. 1.

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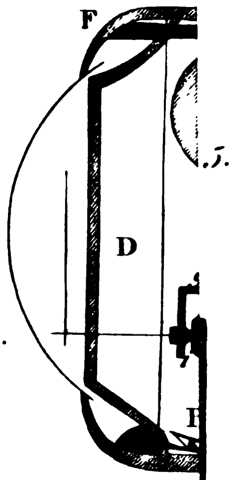


Fig. 3.

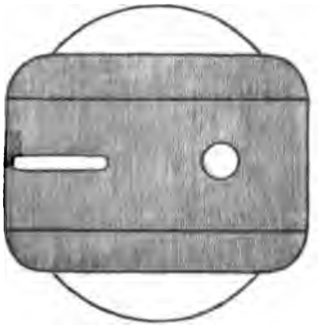


Fig. 7.

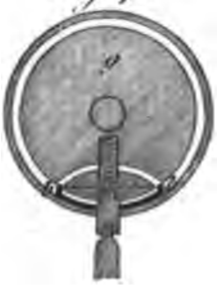


Fig. 10.

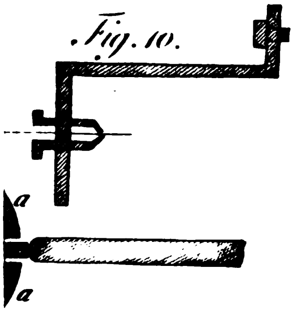
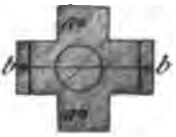


Fig. 11.



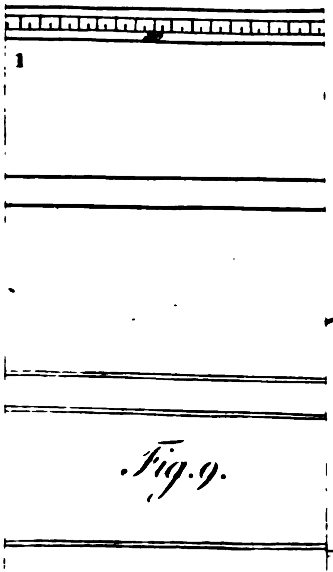
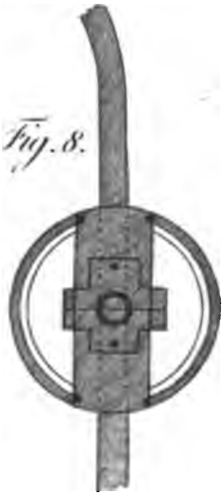


Fig. 9.

Fig. 8.



XXV. *A new Method of inveſtigating the Sums of infinite Series. By the Rev.* S. VINCE, *A. M. of* CAMBRIDGE, *in a Letter to* Henry Maty, *A. M. Secretary.*

Read June 6, 1782.

SIR,

HAVING lately diſcovered ſome very eaſy methods of inveſtigating the ſums of certain infinite ſeries, I have taken the liberty of requeſting the favour of you to preſent them to the Royal Society. I have divided the ſubject into three parts: the firſt contains a new and general method of finding the ſum of thoſe ſeries which DE MOIVRE has found in one or two particular caſes; but whoſe method, although it be in appearance general, will, upon trial, be found to be abſolutely impracticable. The ſecond contains the ſummation of certain ſeries, the laſt differences of whoſe numerators become equal to nothing. The third contains obſervations on a *correction* which is neceſſary in inveſtigating the ſums of certain ſeries by collecting two terms into one, with its application to a variety of caſes.

I am, &c.

Cambridge,
May 3, 1782.

PART

PART I.

LEM. I.

Let r be any whole number, and then the fluent of $\frac{\dot{x}}{1+x^r}$ *can always be exhibited by circular arcs and logarithms; but when* $x=1$, *the fluent of the ſame fluxion will be expreſſed by the infinite ſeries* $1-\frac{1}{r+1}+\frac{1}{2r+1}-\frac{1}{3r+1}+$ &c. *the ſum of this ſeries therefore can always be found by circular arcs and logarithms.*

LEM. II.

To find the ſum of the infinite ſeries $\frac{a}{1\cdot\overline{r+1}}-\frac{a+b}{\overline{r+1}\cdot\overline{2r+1}}+\frac{a+2b}{\overline{2r+1}\cdot\overline{3r+1}}-$ &c.

Aſſume $1-\frac{1}{r+1}+\frac{1}{2r+1}-\frac{1}{3r+1}+$ &c. . $=\mathrm{S}$; therefore,

$$(\mathrm{A})\ 1-\frac{1}{1\cdot\overline{r+1}}+\frac{r+1}{\overline{r+1}\cdot\overline{2r+1}}-\frac{2r+1}{\overline{2r+1}\cdot\overline{3r+1}}+\text{\&c.}\ldots=\mathrm{S}.$$

In the firſt ſeries, add together the 1ſt and 2d, the 2d and 3d, &c. &c. terms, and the reſulting ſeries will evidently be equal to twice that ſeries *minus* the firſt term; therefore,

$$(\mathrm{B}),\ \frac{r}{1\cdot\overline{r+1}}-\frac{r}{\overline{r+1}\cdot\overline{2r+1}}+\frac{r}{\overline{2r+1}\cdot\overline{3r+1}}-\text{\&c.}\ldots=2\mathrm{S}-1.$$

$$\text{Now}\left(\frac{\mathrm{A}}{r}\right)\frac{1}{r}-\frac{\frac{1}{r}}{1\cdot\overline{r+1}}+\frac{1+\frac{1}{r}}{\overline{r+1}\cdot\overline{2r+1}}-\frac{2+\frac{1}{r}}{\overline{2r+1}\cdot\overline{3r+1}}+\text{\&c.}\ldots=\frac{\mathrm{S}}{r},$$

$$\text{or}\left.\begin{array}{l}\frac{1}{\overline{r+1}\cdot\overline{2r+1}}-\frac{2}{\overline{2r+1}\cdot\overline{3r+1}}+\text{\&c.}\ldots\\+\frac{1}{r}-\frac{\frac{1}{r}}{1\cdot\overline{r+1}}+\frac{\frac{1}{r}}{\overline{r+1}\cdot\overline{2r+1}}-\text{\&c.}\ldots\end{array}\right\}=\frac{\mathrm{S}}{r};$$

Now the ſum of the lower ſeries, omitting the firſt term, is equal to $-$B divided by r^2, or $= -\frac{2S-1}{r^2}$; hence, by tranſpoſition, and, multiplying both ſides by b, we ſhall have,

$$\frac{b}{\overline{r+1}\,.\,\overline{2r+1}} - \frac{2b}{\overline{2r+1}\,.\,\overline{3r+1}} + \&c.\ldots = \frac{bS}{r} + \frac{2bS-\overline{r+1}\,.\,b}{r^2};$$ alſo by multiplying B by $\frac{a}{r}$ we have

$$\frac{a}{1\,.\,\overline{r+1}} - \frac{a}{\overline{r+1}\,.\,\overline{2r+1}} + \frac{a}{\overline{2r+1}\,.\,\overline{3r+1}} - \&c.\ldots = \frac{2aS-a}{r};$$ ſubtract the laſt equation but one from the laſt, and we ſhall have

$$\frac{a}{1\,.\,\overline{r+1}} - \frac{a+b}{\overline{r+1}\,.\,\overline{2r+1}} + \frac{a+2b}{\overline{2r+1}\,.\,\overline{3r+1}} - \&c.\ldots = \frac{\overline{2ra-\overline{r+2}\,.\,b}\times S-ra+\overline{r+1}\,.\,b}{r^2}.$$

Cor. 1. Hence it appears, that the ſum of this ſeries can never be exhibited in finite terms, except $a:b$ as $r+2:2r$, in which caſe the ſum is equal to $\frac{a}{r+2}$.

Hence, if $a=3$, $b=2$, then $r=1$; $\therefore \frac{3}{1.2} - \frac{5}{2.3} + \frac{7}{3.4} - \&c.\ldots = 1$;

if $a=1$, $b=4$, then $r=\frac{4}{3}$; $\therefore \frac{5}{3.7} - \frac{9}{7.11} + \frac{13}{11.15} - \frac{17}{15.19} + \&c.\ldots = \frac{1}{6}$;

if $a=4$, $b=3$, then $r=\frac{6}{5}$; $\therefore \frac{4}{5.11} - \frac{7}{11.17} + \frac{10}{17.23} - \frac{13}{23.29} + \&c.\ldots = \frac{1}{20}$;

Cor 2. Put $a=c-b$, and we ſhall have, after tranſpoſition,

$$\frac{c}{\overline{r+1}\,.\,\overline{2r+1}} \quad \frac{c+b}{\overline{2r+1}\,.\,\overline{3r+1}} + \&c.\ldots = \frac{\overline{\overline{3r+2}\,.\,b-2rc}\times S-\overline{2r+1}\ \ b+rc}{r^2} + \frac{c-b}{r+1}.$$

P R O P. I.

To find the ſum of the infinite ſeries $\frac{m}{1\,.\,\overline{r+1}\,.\,\overline{2r+1}} + \frac{m+n}{\overline{2r+1}\,.\,\overline{3r+1}\,.\,\overline{4r+1}} - \frac{m+2n}{\overline{4r+1}\,.\,\overline{5r+1}\,.\,\overline{6r+1}} + \&c.$

Every ſeries of this kind may be reſolved into the following ſeries $\frac{a}{1 . r+1} - \frac{a+b}{r+1 . 2r+1} + \frac{a+2b}{2r+1 . 3r+1} - \frac{a+3b}{3r+1 . 4r+1} +$ &c. for if we reduce two terms of this ſeries into one, it will become $\frac{2ar-b}{1 . r+1 . 2r+1} + \frac{2ra+\overline{2r-1} . b}{2r+1 . 3r+1 . 4r+1} + \frac{2ra+\overline{4r-1} . b}{4r+1 . 5r+1 . 6r+1} +$ &c. where the denominators being the ſame as in the given ſeries, and the numerators alſo in arithmetic progreſſion, we have only to take a and b ſuch quantities that the reſpective numerators may be alſo equal; aſſume, therefore, $2ra-b=m$, $2ra+\overline{2r-1} . b=m+n$; therefore, $b=\frac{n}{2r}$, $a=\frac{2rm+n}{4r^2}$, which ſubſtituted for a and b in LEM. 2. gives

$$\frac{m}{1 . r+1 . 2r+1} + \frac{m+n}{2r+1 . 3r+1 . 4r+1} + \frac{m+2n}{4r+1 . 5r+1 . 6r+1} + \&c. \ldots$$
$$= \frac{2rm-\overline{r+1} . n}{2r^3} \times S + \frac{\overline{2r+1} . n-2rm}{4r^3}.$$

Let $r=1$, and we have

$$\frac{m}{1 . 2 . 3} + \frac{m+n}{3 . 4 . 5} + \frac{m+2n}{5 . 6 . 7} + \&c. \ldots = \overline{m-n} . S + \frac{3n-2m}{4}.$$

If $m=1$, $n=3$, $\frac{1}{1 . 2 . 3} + \frac{4}{3 . 4 . 5} + \frac{7}{5 . 6 . 7} + \&c. \ldots = \frac{7}{4} - 2S$;

$m=1$, $n=0$, $\frac{1}{1 . 2 . 3} + \frac{1}{3 . 4 . 5} + \frac{1}{5 . 6 . 7} + \&c. \ldots = S - \frac{1}{2}$.

Let $r=2$, and the ſeries becomes

$$\frac{m}{1 . 3 . 5} + \frac{m+n}{5 . 7 . 9} + \frac{m+2n}{9 . 11 . 13} + \&c. \ldots = \frac{4m-3n}{16} \times S + \frac{5n-4m}{32}.$$

If $m=1$, $n=1$, $\frac{1}{1 . 3 . 5} + \frac{2}{5 . 7 . 9} + \frac{3}{9 . 11 . 13} + \&c. \ldots = \frac{S}{16} + \frac{1}{32}$;

$m=1$, $n=0$, $\frac{1}{1 . 3 . 5} + \frac{1}{5 . 7 . 9} + \frac{1}{9 . 11 . 13} + \&c. \ldots = \frac{S}{4} - \frac{1}{8}$.

Let $r=5$, and we ſhall have

$$\frac{m}{1 . 6 . 11} + \frac{m+n}{11 . 16 . 21} + \frac{m+2n}{21 . 26 . 31} + \&c. \ldots = \frac{5m-3n}{125} \times S + \frac{11n-10m}{500}.$$

If $m=1$, $n=1$, $\frac{1}{1.6.11}+\frac{2}{11.16.21}+\frac{3}{21.26.31}+\&c.\ldots=\frac{2}{125}\times S+\frac{1}{500}$;

$m=1, n=0, \frac{1}{1.6.11}+\frac{1}{11.16.21}+\frac{1}{21.26.31}+\&c.\ldots=\frac{S}{25}-\frac{1}{50}.$

Cor. If $2r : r+1 :: n : m$, the ſum of the ſeries can be accurately found, and will be equal to $\frac{m}{2r\,.\,\overline{r+1}}$. Let therefore $m=r+1$, and then $n=2r$, conſequently

$$\frac{1}{1\,.\,\overline{2r+1}}+\frac{1}{\overline{2r+1}\,.\,\overline{4r+1}}+\frac{1}{\overline{4r+1}\,.\,\overline{6r+1}}+\&c.\ldots=\frac{1}{2r};$$

which is alſo known from other principles.

P R O P. II.

To find the ſum of the infinite ſeries $\frac{m}{\overline{r+1}\,.\,\overline{2r+1}\,.\,\overline{3r+1}}+\frac{m+n}{\overline{3r+1}\,.\,\overline{4r+1}\,.\,\overline{5r+1}}+\frac{m+2n}{\overline{5r+1}\,.\,\overline{6r+1}\,.\,\overline{7r+1}}+\&c.$

This ſeries reſolves itſelf into

$$\frac{c}{\overline{r+1}\,.\,\overline{2r+1}}-\frac{c+b}{\overline{2r+1}\,.\,\overline{3r+1}}+\frac{c+2b}{\overline{3r+1}\,.\,\overline{4r+1}}-\&c.;$$

for by reduction, as before, it becomes

$$\frac{2cr-\overline{r+1}\,.\,b}{\overline{r+1}\,.\,\overline{2r+1}\,.\,\overline{3r+1}}+\frac{2cr+\overline{r-1}\,.\,b}{\overline{3r+1}\,.\,\overline{4r+1}\,.\,\overline{5r+1}}+\frac{2cr+\overline{3r-1}\,.\,b}{\overline{5r+1}\,.\,\overline{6r+1}\,.\,\overline{7r+1}}+\&c.$$

where the denominators are the ſame as in the given ſeries, and the numerators in arithmetic progreſſion; aſſume therefore $2cr-\overline{r+1}\,.\,b=m$, $2cr+\overline{r-1}\,.\,b=m+n$, hence $b=\frac{n}{2r}$, $c=\frac{2rm+\overline{r+1}\,.\,n}{4r^2}$, which, ſubſtituted in cor. 2. LEM. 2. give

$$\frac{m}{\overline{r+1}\,.\,\overline{2r+1}\,.\,\overline{3r+1}}+\frac{m+n}{\overline{3r+1}\,.\,\overline{4r+1}\,.\,\overline{5r+1}}+\frac{m+3n}{\overline{5r+1}\,.\,\overline{6r+1}\,.\,\overline{7r+1}}+\&c.\ldots$$

$$=\frac{\overline{2r+1}\,.\,n-2rm}{2r^3}\times S+\frac{2rm-\overline{3r+1}\,.\,n}{4r^3}+\frac{2rm-\overline{r-1}\,.\,n}{4r^2\,.\,\overline{r+1}}.$$

Cor. 1. In prop. 1. ſubſtitute a for m, and $2b$ for n, and we have

$$\frac{a}{1\,.\,\overline{r+1}\,.\,\overline{2r+1}} + \frac{a+2b}{\overline{2r+1}\,.\,\overline{3r+1}\,.\,\overline{4r+1}} + \frac{a+4b}{\overline{4r+1}\,.\,\overline{5r+1}\,.\,\overline{6r+1}} + \&c.\ldots = \frac{ra-\overline{r+1}\,.\,b}{r^3}\times S + \frac{\overline{2r+1}\,.\,b-ra}{2r^3}.$$

Alſo in this prop. ſubſtitute $a+b$ for m, and $2b$ for n, and we have

$$\frac{a+b}{\overline{r+1}\,.\,\overline{2r+1}\,.\,\overline{3r+1}} + \frac{a+3b}{\overline{3r+1}\,.\,\overline{4r+1}\,.\,\overline{5r+1}} + \&c.\ldots = \frac{\overline{r+1}\,.\,b-ra}{r^3}\times S + \frac{ra-\overline{2r+1}\,.\,b}{2r^3} + \frac{ra+b}{2r^2\times\overline{r+1}}.$$

Subtract this latter ſeries from the former, and

$$\frac{a}{1\,.\,\overline{r+1}\,.\,\overline{2r+1}} - \frac{a+b}{\overline{r+1}\,.\,\overline{2r+1}\,.\,\overline{3r+1}} + \frac{a+2b}{\overline{2r+1}\,.\,\overline{3r+1}\,.\,\overline{4r+1}} - \&c.\ldots = \frac{2ra-\overline{r+1}\,.\,2b}{r^3}\times S + \frac{\overline{2r+1}\,.\,b-ra}{r^3} - \frac{ra+2b}{2r^2\times\overline{r+1}}.$$

Let $r=1$, and we have

$$\frac{a}{1\,.\,2\,.\,3} - \frac{a+b}{2\,.\,3\,.\,4} + \frac{a+2b}{3\,.\,4\,.\,5} - \&c.\ldots = \overline{2a-4b}\times S + \frac{11b-5a}{4}.$$

If $a=1$, $b=0$, $\frac{1}{1\,.\,2\,.\,3} - \frac{1}{2\,.\,3\,.\,4} + \frac{1}{3\,.\,4\,.\,5} - \&c.\ldots = 2S - \frac{5}{4}$;

$a=1$, $b=2$, $\frac{1}{1\,.\,2\,.\,3} - \frac{3}{2\,.\,3\,.\,4} + \frac{5}{3\,.\,4\,.\,5} - \&c.\ldots = \frac{17}{4} - 6S.$

Let $r=3$, and we have

$$\frac{a}{1\,.\,4\,.\,7} - \frac{a+b}{4\,.\,7\,.\,10} + \frac{a+2b}{7\,.\,10\,.\,13} - \&c.\ldots = \frac{6a-8b}{27}\times S + \frac{53b-33a}{216}.$$

If $a=1$, $b=0$, $\frac{1}{1\,.\,4\,.\,7} - \frac{1}{4\,.\,7\,.\,10} + \frac{1}{7\,.\,10\,.\,13} - \&c.\ldots = \frac{2}{9}S - \frac{11}{72}$;

$a=1$, $b=1$, $\frac{1}{1\,.\,4\,.\,7} - \frac{2}{4\,.\,7\,.\,10} + \frac{3}{7\,.\,10\,.\,13} - \&c.\ldots = \frac{5}{54} - \frac{2}{27}S.$

If, inſtead of ſubſtituting in prop. 1. $2b$ and a for n and m, we had ſubſtituted two other quantities, as $2r$ and s, and then proceeded as above, a ſeries would have been formed, the numerators of whoſe alternate terms would have formed each a a ſeparate arithmetic progreſſion.

If the latter ſeries had been *added* to the former a ſeries would have been formed whoſe terms would have been all poſitive; but as I purpoſe, in the ſecond part of this paper, to give a general method of ſumming all ſuch ſeries, I ſhall not ſtop here to apply this method of inveſtigation.

Cor. 2. In propoſition 2. ſubſtitute a for m, and $2b$ for n, and we ſhall have

$$\frac{a}{\overline{r+1}\cdot\overline{2r+1}\cdot\overline{3r+1}}+\frac{a+2b}{\overline{3r+1}\cdot\overline{4r+1}\cdot\overline{5r+1}}+\&c.\ldots=$$

$$\frac{\overline{2r+1}\cdot b-ra}{r^3}\times S+\frac{ra-\overline{3r+1}\cdot b}{2r^3}+\frac{ra-\overline{r-1}\cdot b}{2r^2\cdot\overline{r+1}}.$$

Alſo in prop. 1. write $a-b$ for m, and $2b$ for n, and there reſults

$$\frac{a+b}{\overline{2r+1}\cdot\overline{3r+1}\cdot\overline{4r+1}}+\frac{a+3b}{\overline{4r+1}\cdot\overline{5r+1}\cdot\overline{6r+1}}+\&c.\ldots=$$

$$\frac{ra-\overline{2r+1}\cdot b}{r^3}\times S+\frac{\overline{r+1}\cdot b-ra}{2r^3}-\frac{a-b}{\overline{r+1}\cdot\overline{2r+1}}.$$

Subtract this latter ſeries from the former, and we ſhall have

$$\frac{a}{\overline{r+1}\cdot\overline{2r+1}\cdot\overline{3r+1}}-\frac{a+b}{\overline{2r+1}\cdot\overline{3r+1}\cdot\overline{4r+1}}+\frac{a+2b}{\overline{3r+1}\cdot\overline{4r+1}\cdot\overline{5r+1}}-\&c.\ldots$$

$$=\frac{\overline{2r+1}\cdot 2b-2ra}{r^3}\times S+\frac{ra-\overline{3r+1}\cdot b}{r^3}+\frac{ra-\overline{r-1}\cdot b}{2r^2\cdot\overline{r+1}}+\frac{a-b}{\overline{r+1}\cdot\overline{2r+1}}.$$

P R O P. III.

To find the ſum of the infinite ſeries $\frac{m}{1\cdot\overline{r+1}\cdot\overline{2r+1}\cdot\overline{3r+1}}+$

$$\frac{m+n}{\overline{2r+1}\cdot\overline{3r+1}\cdot\overline{4r+1}\cdot\overline{5r+1}}+\frac{m+2n}{\overline{4r+1}\cdot\overline{5r+1}\cdot\overline{6r+1}\cdot\overline{7r+1}}+\&c.$$

This ſeries reſolves itſelf into

$$\frac{a}{1\cdot\overline{r+1}\cdot\overline{2r+1}}-\frac{a+b}{\overline{r+1}\cdot\overline{2r+1}\cdot\overline{3r+1}}+\frac{a+2b}{\overline{2r+1}\cdot\overline{3r+1}\cdot\overline{4r+1}}-\&c.$$

for by reduction it becomes

$$\frac{3ra-b}{1.\overline{r+1}.\overline{2r+1}.\overline{3r+1}}+\frac{3ar+\overline{4r-1}.b}{\overline{2r+1}.\overline{3r+1}.\overline{4r+1}.\overline{5r+1}}+$$
$$\frac{3ar+\overline{8r-1}.b}{\overline{4r+1}.\overline{5r+1}.\overline{6r+1}.\overline{7r+1}}+\&c.$$

where the numerators are in arithmetic progreſſion, and the denominators the ſame as in the given ſeries; aſſume therefore $3ra-b=m$, $3ra+\overline{4r-1}.b=m+n$, hence $b=\frac{n}{4r}$, $a=\frac{4rm+n}{12r^2}$; ſubſtitute theſe values into cor. 1. prop. 2. and we have

$$\frac{m}{1.\overline{r+1}.\overline{2+1}.\overline{3r+1}}+\frac{m+n}{\overline{2r+1}.\overline{3r+1}.\overline{4r+1}.\overline{5r+1}}+$$
$$\frac{m+2n}{\overline{4r+1}.\overline{5r+1}.\overline{6r+1}.\overline{7r+1}}+\&c.\ldots=\frac{4rm-\overline{3r+2}.n}{6r^4}\times S+$$
$$\frac{\overline{3r+1}.n-2rm}{6r^4}-\frac{rm+n}{6r^3.\overline{r+1}}.$$

Let $r=1$, and we have

$$\frac{m}{1.2.3.4}+\frac{m+n}{3.4.5.6}+\frac{m+2n}{5.6.7.8}+\&c.\ldots=\frac{4m-5n}{6}\times S+\frac{7n-5m}{12}.$$

If $m=1, n=1$, $\frac{1}{1.2.3.4}+\frac{1}{3.4.5.6}+\frac{1}{5.6.7.8}+\&c.\ldots=\frac{2}{3}S-\frac{5}{12}$;

$m=1, n=1$, $\frac{1}{1.2.3.4}+\frac{2}{3.4.5.6}+\frac{3}{5.6.7.8}+\&c.\ldots=\frac{1}{6}-\frac{1}{6}S$;

$m=7, n=5$, $\frac{7}{1.2.3.4}+\frac{12}{3.4.5.6}+\frac{17}{5.6.7.8}+\&c.\ldots=\frac{1}{2}S$;

Let $r=2$, and we have

$$\frac{m}{1.3.5.7}+\frac{m+n}{5.7.9.11}+\frac{m2n}{9.11.13.15}+\&c.\ldots=\frac{m-n}{12}\times S-\frac{19n-16m}{288}.$$

If $m=1, n=3$, $\frac{1}{1.3.5.7}+\frac{4}{5.7.9.11}+\frac{7}{9.11.13.15}+\&c.\ldots=\frac{41}{288}-\frac{1}{6}S$;

$m=1, n=0$, $\frac{1}{1.3.5.7}+\frac{1}{5.7.9.11}+\frac{1}{9.11.13.15}+\&c.\ldots=\frac{1}{12}S-\frac{1}{18}$.

$m=19, n=16$. $\frac{19}{1.3.5.7}+\frac{35}{5.7.9.11}+\frac{51}{9.11.13.15}+\&c.\ldots=\frac{1}{4}S$,

Cor. If $n:m$ as $4r:3r+2$, the ſum of the ſeries can be accurately had; let therefore $n=4r$ and $m=3r+2$, and we ſhall have

$$\frac{3r+2}{1.\overline{r+1}.\overline{2r+1}.\overline{3r+1}}+\frac{7r+2}{\overline{2r+1}.\overline{3r+1}.\overline{4r+1}.\overline{5r+1}}+\&c.\ldots=\frac{1}{2r.\overline{r+1}}.$$

If $r=1$, $\frac{5}{1.2.3.4}+\frac{9}{3.4.5.6}+\frac{13}{5.6.7.9}+\&c.\ldots=\frac{1}{4}$ *;

$r=2$, $\frac{1}{1.3.5.7}+\frac{2}{5.7.9.11}+\frac{3}{9.11.13.15}+\&c.\ldots=\frac{1}{96}$;

$r=6$, $\frac{5}{1.7.13.19}+\frac{11}{13.19.25.31}+\frac{17}{25.31.37.43}+\&c.\ldots=\frac{1}{336}$.

P R O P. IV.

To find the ſum of the infinite ſeries $\frac{m}{\overline{r+1}.\overline{2r+1}.\overline{3r+1}.\overline{4r+1}}+\frac{m+n}{\overline{3r+1}.\overline{4r+1}.\overline{5r+1}.\overline{6r+1}}+\frac{m+2n}{\overline{5r+1}.\overline{6r+1}.\overline{7r+1}.\overline{8r+1}}+\&c.$

This ſeries reſolves itſelf into $\frac{a}{\overline{r+1}.\overline{2r+1}.\overline{3r+1}}-\frac{a+b}{\overline{2r+1}.\overline{3r+1}.\overline{4r+1}}+\frac{a+2b}{\overline{3r+1}.\overline{4r+1}.\overline{5r+1}}-\&c.$ for by reduction it becomes $\frac{3ra-\overline{r+1}.b}{\overline{r+1}.\overline{2r+1}.\overline{3r+1}.\overline{4r+1}}+\frac{3ra+\overline{3r-1}.b}{\overline{3r+1}.\overline{4r+1}.\overline{5r+1}.\overline{6r+1}}+\frac{3ra+\overline{7r-1}.b}{\overline{5r+1}.\overline{6r+1}.\overline{7r+1}.\overline{8r+1}}+\&c.$ where the denominators are the ſame as in the given ſeries, and the numerators alſo in arithmetic progreſſion; put therefore $3ra-\overline{r+1}.b=m$, $3ra+\overline{3r-1}.b=m+n$, hence $b=\frac{n}{4r}$, $a=\frac{4rm+\overline{r+1}.n}{12r^2}$, which, ſubſtituted in cor. 2. prop. 2. give

$$\frac{m}{\overline{r+1}.\overline{2r+1}.\overline{3r+1}.\overline{4r+1}}+\frac{m+n}{\overline{3r+1}.\overline{4r+1}.\overline{5r+1}.\overline{6r+1}}+\&c.\ldots=\frac{\overline{5r+2}.n-4rm}{6r^4}\times S+\frac{2rm-\overline{4r+1}.n}{6r^4}+\frac{2rm-\overline{r-2}.n}{12.r^3.\overline{r+1}}+\frac{4rm-\overline{2r-1}.n}{12r^2.\overline{r+1}.\overline{2r+1}}.$$

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* Vide DE MOIVRE's Miſ. Anal. pag. 134.

Cor. 1. In prop. 3. write a for m and $2b$ for n, and we have

$$\frac{a}{1\cdot\overline{r+1}\cdot\overline{2r+1}\cdot\overline{3r+1}}+\frac{a+2b}{\overline{2r+1}\cdot\overline{3r+1}\cdot\overline{4r+1}\cdot\overline{5r+1}}+\&c.\ldots=$$

$$\frac{2ra-\overline{3r+2}\cdot b}{3r^4}\times S+\frac{\overline{3r+1}\cdot b-ra}{3r^4}-\frac{ra+2b}{6r^3\cdot\overline{r+1}}.$$

Alſo in this prop. write $a+b$ for m, and $2b$ for n, and we have

$$\frac{a+b}{\overline{r+1}\cdot\overline{2r+1}\cdot\overline{3r+1}\cdot\overline{4r+1}}+\frac{a+3b}{\overline{3r+1}\cdot\overline{4r+1}\cdot\overline{5r+1}\cdot\overline{6r+1}}+\&c.\ldots=$$

$$\frac{\overline{3r+2}\cdot b-2ra}{3r^4}\times S+\frac{ra-\overline{3r+1}\cdot b}{3r^4}+\frac{ra+2b}{6r^3\cdot\overline{r+1}}+\frac{2ra+b}{6r^2\cdot\overline{r+1}\cdot\overline{2r+1}}.$$

ſubtract this latter ſeries from the former, and we have

$$\frac{a}{1\cdot\overline{r+1}\cdot\overline{2r+1}\cdot\overline{3r+1}}-\frac{a+b}{\overline{r+1}\cdot\overline{2r+1}\cdot\overline{3r+1}\cdot\overline{4r+1}}+\&c.\ldots=$$

$$\frac{4ra-\overline{3r+2}\cdot 2b}{3r^4}\times S+\frac{\overline{3r+1}\cdot 2b-2ra}{3r^4}-\frac{ra+2b}{3r^3\cdot\overline{r+1}}-\frac{2ra+b}{6r^2\cdot\overline{r+1}\cdot\overline{2r+1}}.$$

Let $r=1$, and we have

$$\frac{a}{1\cdot2\cdot3\cdot4}-\frac{a+b}{2\cdot3\cdot4\cdot5}+\frac{a+2b}{3\cdot4\cdot5\cdot6}-\&c.\ldots=\frac{4a-10b}{3}\times S+\frac{83b-32a}{36}.$$

If $a=1$, $b=0$, $\frac{1}{1\cdot2\cdot3\cdot4}-\frac{1}{2\cdot3\cdot4\cdot5}+\frac{1}{3\cdot4\cdot5\cdot6}-\&c.\ldots=\frac{4}{3}S-\frac{8}{9}$.

$a=3$, $b=1$, $\frac{1}{1\cdot2\cdot4}-\frac{1}{2\cdot3\cdot5}+\frac{1}{3\cdot4\cdot6}-\&c.\ldots=\frac{2}{3}S-\frac{13}{36}$.

Let $r=3$, and we have

$$\frac{a}{1\cdot4\cdot7\cdot10}-\frac{a+b}{4\cdot7\cdot10\cdot13}+\frac{a+2b}{7\cdot10\cdot13\cdot16}-\&c.\ldots=\frac{12a-22b}{243}\times S+\frac{1027b-156a}{3\cdot81\cdot56}$$

If $a=1$, $b=1$, $\frac{1}{1\cdot4\cdot7\cdot10}-\frac{2}{4\cdot7\cdot10\cdot13}+\frac{3}{7\cdot10\cdot13\cdot16}-\&c.\ldots=\frac{871}{3\cdot81\cdot56}-\frac{10}{3\cdot81}S$.

$a=4$, $b=3$, $\frac{1}{1\cdot7\cdot10}-\frac{1}{4\cdot10\cdot13}+\frac{1}{7\cdot13\cdot16}-\&c.\ldots=\frac{13}{72}-\frac{2}{27}S$.

Cor. 2. If $a:b$ as $3r+2:2r$, the ſum of the ſeries can be accurately found; take $\therefore a=3r+2$, and $b=2r$, and we ſhall have

$$\frac{3r+2}{1\cdot\overline{r+1}\cdot\overline{2r+1}\cdot\overline{3r+1}}-\frac{5r+2}{\overline{r+1}\cdot\overline{2r+1}\cdot\overline{3r+1}\cdot\overline{4r+1}}+\&c.\ldots=\frac{1}{\overline{r+1}\cdot\overline{2r+1}}.$$

If

$$\text{If } r=1,\ \frac{5}{1\cdot2\cdot3\cdot4}-\frac{7}{2\cdot3\cdot4\cdot5}+\frac{9}{3\cdot4\cdot5\cdot6}-\&c.\ldots=\frac{1}{60};$$

$$r=2,\ \frac{2}{1\cdot3\cdot5\cdot7}-\frac{3}{3\cdot5\cdot7\cdot9}+\frac{4}{5\cdot7\cdot9\cdot11}-\&c.\ldots=\frac{1}{15};$$

$$r=3,\ \frac{11}{1\cdot4\cdot7\cdot10}-\frac{17}{4\cdot7\cdot10\cdot13}+\frac{23}{7\cdot10\cdot13\cdot16}-\&c.\ldots=\frac{1}{28}.$$

PROP. V.

To find the ſum of the infinite ſeries $\frac{m}{1\cdot\overline{r+1}\cdot\overline{2r+1}\cdot\overline{3r+1}\cdot\overline{4r+1}}+\frac{m+n}{\overline{2r+1}\cdot\overline{3r+1}\cdot\overline{4r+1}\cdot\overline{5r+1}\cdot\overline{6r+1}}+\frac{m+2n}{\overline{4r+1}\cdot\overline{5r+1}\cdot\overline{6r+1}\cdot\overline{7r+1}\cdot\overline{8r+1}}+\&c.$

This ſeries reſolves itſelf into $\frac{a}{1\cdot\overline{r+1}\cdot\overline{2r+1}\cdot\overline{3r+1}}-\frac{a+b}{\overline{r+1}\cdot\overline{2r+1}\cdot\overline{3r+1}\cdot\overline{4r+1}}+\frac{a+2b}{\overline{2r+1}\cdot\overline{3r+1}\cdot\overline{4r+1}\cdot\overline{5r+1}}-\&c.$; for by reduction this ſeries becomes $\frac{4ra-b}{1\cdot\overline{r+1}\cdot\overline{2r+1}\cdot\overline{3r+1}\cdot\overline{4r+1}}+\frac{4ra+\overline{6r-1}\cdot b}{\overline{2r+1}\cdot\overline{3r+1}\cdot\overline{4r+1}\cdot\overline{5r+1}\cdot\overline{6r+1}}+\frac{4ra+\overline{12r-1}\cdot b}{\overline{4r+1}\cdot\overline{5r+1}\cdot\overline{6r+1}\cdot\overline{7r+1}\cdot\overline{8r+1}}+\&c.$ where the numerators are in arithmetic progreſſion, and the denominators the ſame as in the given ſeries; aſſume therefore $4ra-b=m$, $4ra+\overline{6r-1}\cdot b=m+n$, hence $b=\frac{n}{6r}$, $a=\frac{6rm+n}{24r^2}$, which values being ſubſtituted in cor. 1. prop. 4. give

$$\frac{m}{1\cdot\overline{r+1}\cdot\overline{2r+1}\cdot\overline{3r+1}\cdot\overline{4r+1}}+\frac{m+n}{\overline{2r+1}\cdot\overline{3r+1}\cdot\overline{4r+1}\cdot\overline{5r+1}\cdot\overline{6r+1}}+\&c.\ldots=$$
$$\frac{2rm-\overline{2r+1}\cdot n}{6r^5}\times S+\frac{\overline{4r+1}\cdot n-2rm}{12r^3}-\frac{6rm+9n}{72\cdot r^4\cdot\overline{r+1}}-\frac{2rm+n}{24r^3\cdot\overline{r+1}\cdot\overline{2r+1}}.$$

Let $r=1$, and we have

$$\frac{m}{1\cdot2\cdot3\cdot4\cdot5}+\frac{m+n}{3\cdot4\cdot5\cdot6\cdot7}+\frac{m+2n}{5\cdot6\cdot7\cdot8\cdot9}+\&c.\ldots=\frac{2m-3n}{6}\times S+\frac{25n-16m}{72}.$$

It

If $m=1, n=0, \frac{1}{1\cdot 2\cdot 3\cdot 4\cdot 5}+\frac{1}{3\cdot 4\cdot 5\cdot 6\cdot 7}+\frac{1}{5\cdot 6\cdot 7\cdot 8\cdot 9}+\&c.\ldots=\frac{1}{3}S-\frac{2}{9};$

$m=1, n=1, \frac{1}{1\cdot 2\cdot 3\cdot 4\cdot 5}+\frac{2}{3\cdot 4\cdot 5\cdot 6\cdot 7}+\frac{3}{5\cdot 6\cdot 7\cdot 8\cdot 9}+\&c.\ldots=\frac{1}{8}-\frac{1}{6}S;$

$m=4, n=2, \frac{1}{1\cdot 2\cdot 3\cdot 5}+\frac{1}{3\cdot 4\cdot 5\cdot 7}+\frac{1}{5\cdot 6\cdot 7\cdot 9}+\&c.\ldots=\frac{1}{3}S-\frac{7}{36}.$

$m=25, n=16, \frac{25}{1\cdot 2\cdot 3\cdot 4\cdot 5}+\frac{41}{3\cdot 4\cdot 5\cdot 6\cdot 7}+\frac{57}{5\cdot 6\cdot 7\cdot 8\cdot 9}+\&c.\ldots=\frac{1}{3}S.$

Let $r=2$, and we have $\frac{m}{1\cdot 3\cdot 5\cdot 7\cdot 9}+\frac{m+n}{5\cdot 7\cdot 9\cdot 11\cdot 13}+\frac{m+2n}{9\cdot 11\cdot 13\cdot 15\cdot 17}+\&c.\ldots=\frac{4m-5n}{192}\times S+\frac{59n-44m}{24\cdot 120}.$

If $m=1, n=0, \frac{1}{1\cdot 3\cdot 5\cdot 7\cdot 9}+\frac{1}{5\cdot 7\cdot 9\cdot 11\cdot 13}+\frac{1}{9\cdot 11\cdot 13\cdot 15\cdot 17}+\&c.\ldots=\frac{1}{48}S-\frac{11}{24\cdot 30}.$

$m=3, n=2, \frac{1}{1\cdot 5\cdot 7\cdot 9}+\frac{1}{5\cdot 9\cdot 11\cdot 13}+\frac{1}{9\cdot 13\cdot 15\cdot 17}+\&c.\ldots=\frac{1}{96}S-\frac{7}{24\cdot 60}.$

Cor. If $n:m$ as $2r:2r+1$, the ſum of the ſeries can be accurately found; aſſume therefore $n=2r$, $m=2r+1$, and we have

$$\frac{1}{1\cdot\overline{r+1}\cdot\overline{3r+1}\cdot\overline{4r+1}}+\frac{1}{\overline{2r+1}\cdot\overline{3r+1}\cdot\overline{5r+1}\cdot\overline{6r+1}}+\&c.\ldots=\frac{1}{6\cdot r\cdot\overline{r+1}\cdot\overline{2r+1}}.$$

If $r=1, \frac{1}{1\cdot 2\cdot 4\cdot 5}+\frac{1}{3\cdot 4\cdot 6\cdot 7}+\frac{1}{5\cdot 6\cdot 8\cdot 9}+\&c.\ldots=\frac{1}{36};$

$r=3, \frac{1}{1\cdot 4\cdot 10\cdot 13}+\frac{1}{7\cdot 10\cdot 16\cdot 19}+\frac{1}{13\cdot 16\cdot 22\cdot 25}+\&c.\ldots\frac{1}{504}.$

Having thus far explained the method of ſummation of ſuch ſeries as I propoſed to treat of in the firſt part of this paper, I truſt it is not neceſſary to ſay any thing further, as the ſame method of proceeding will manifeſtly continue the ſeries to any propoſed number of factors in the denominator; I ſhall therefore conclude with pointing out a remarkable property of thoſe ſeries whoſe ſum can be accurately found: that when the number of factors in the denominator is *even*, the numerator is always equal to the ſum of the two middle factors; and when the number of factors be *odd*, the numerator will be equal to the middle factor, and conſequently will take it out of the denominator, and leave a ſeries whoſe numerators are unity, and whoſe denominators want the middle factor.

The method of ſummation of ſeries here made uſe of may alſo be applied in inveſtigating the ſums of a great variety of other ſeries; but as a further application of this method would carry us beyond the limits to which this paper muſt be confined, I ſhall re-aſſume the ſubject at ſome future opportunity, and proceed immediately to the ſecond part.

PART II.

PROP.

To find the ſum of the infinite ſeries $\frac{p}{n\,.\,\overline{n+m}\ldots\ldots\overline{n+rm}}+\frac{q}{\overline{n+m}\ldots\ldots\overline{n+\overline{r+1}\,.\,m}}+\frac{s}{\overline{n+2m}\ldots\ldots\overline{n+\overline{r+2}\,.\,m}}+$ &c. *when the laſt differences of the numerators become equal to nothing.*

Aſſume $a+nb+n\,.\,\overline{n+m}\,.\,c+n\,.\,\overline{n+m}\,.\,\overline{n+2m}\,.\,d+$ &c. to any number (r) of terms; then, if for n we write $n+m$, $n+2m$, $n+3m$, &c. ſucceſſively, there will reſult a ſeries of quantities

whoſe

whoſe rth difference is $=0$; ſubſtitute, therefore, this ſeries of quantities for p, q, s, &c. reſpectively, and the given ſeries becomes

$$\frac{a+nb+n.\overline{n+m}.c+\&c.}{n.\overline{n+m}\ldots\overline{n+rm}}+\frac{a+\overline{n+m}.b+\overline{n+m}.\overline{n+2m}.c+\&c.}{\overline{n+m}\ldots\overline{n+r+1}.m}+$$

$$\frac{a+\overline{n+2m}.b+\overline{n+2m}.\overline{n+3m}.c+\&c.}{\overline{n+2m}\ldots\overline{n+r+2}.m}+\&c.$$

which manifeſtly reſolves itſelf into the following ſeries

$$\frac{a}{n.\overline{n+m}\ldots\overline{n+rm}}+\frac{a}{\overline{n+m}\ldots\overline{n+r+1}.m}+\frac{a}{\overline{n+2m}\ldots\overline{n+r+2}.m}+\&c.$$

$$\frac{b}{\overline{n+m}\ldots\overline{n+rm}}+\frac{b}{\overline{n+2m}\ldots\overline{n+r+1}.m}+\frac{b}{\overline{n+3m}\ldots\overline{n+r+2}.m}+\&c.$$

$$\frac{c}{\overline{n+2m}\ldots\overline{n+rm}}+\frac{c}{\overline{n+3m}\ldots\overline{n+r+1}.m}+\frac{c}{\overline{n+4m}\ldots\overline{n+r+2}.m}+\&c.$$

&c. &c. &c.

where the number of ſeries is r, the ſum of each of which being taken by a well known rule, the ſum of the given ſeries becomes

$$\frac{a}{n.\overline{n+m}\ldots\overline{n+r-1}.m.m.r}+\frac{b}{\overline{n+m}\ldots\overline{n+r-1}.m.m.\overline{r-1}}+$$

$$\frac{c}{\overline{n+2m}\ldots\overline{n+r-1}.m.m.\overline{r-2}}+\&c.$$

where the law of continuation is manifeſt.

CASE I. *To find the ſum of the infinite ſeries* $\frac{3}{1.2.3.4}+\frac{6}{2.3.4.5}+\frac{10}{3.4.5.6}+\frac{15}{4.5.6.7}+\&c.$

Here $n=1$, $m=1$, $r=3$, and the third differences become $=0$; therefore $a+b+2c=3$, $a+2b+6c=6$, $a+3b+12c=10$, conſequently $a=1$, $b=1$, $c=\frac{1}{2}$, and therefore the ſum ſought will be $\frac{1}{1.2.3.3}+\frac{1}{2.3.2}+\frac{1}{2.3}=\frac{11}{36}$.

CASE 2. *To find the ſum of the infinite ſeries* $\frac{1 \cdot 2}{1 \cdot 3 \cdot 5 \cdot 7} + \frac{2 \cdot 3}{3 \cdot 5 \cdot 7 \cdot 9} + \frac{3 \cdot 4}{5 \cdot 7 \cdot 9 \cdot 11} +$ &c.

In this caſe $n = 1$, $m = 2$, $r = 3$, and the 3d differences become $= 0$; therefore $a + b + 3c = 2$, $a + 3b + 15c = 6$, $a + 5b + 35c = 12$, conſequently $a = \frac{3}{4}$, $b = \frac{1}{2}$, $c = \frac{1}{4}$, and hence the ſum of the required ſeries becomes $\frac{3}{1 \cdot 3 \cdot 5 \cdot 2 \cdot 3 \cdot 4} + \frac{1}{3 \cdot 5 \cdot 2 \cdot 2 \cdot 2} + \frac{1}{5 \cdot 2 \cdot 4} = \frac{1}{24}$.

CASE 3. *To find the ſum of the infinite ſeries* $\frac{1}{3 \cdot 4 \cdot 5 \cdot 6 \cdot 7 \cdot 8} + \frac{2}{4 \cdot 5 \cdot 6 \cdot 7 \cdot 8 \cdot 9} + \frac{4}{5 \cdot 6 \cdot 7 \cdot 8 \cdot 9 \cdot 10} + \frac{12}{6 \cdot 7 \cdot 8 \cdot 9 \cdot 10 \cdot 11} + \frac{31}{7 \cdot 8 \cdot 9 \cdot 10 \cdot 11 \cdot 12} +$ &c.

Here $n = 3$, $m = 1$, $r = 5$, and the 4th differences become $= 0$; therefore $a + 3b + 12c + 60d = 1$, $a + 4b + 20c + 120d = 2$, $a + 5b + 30c + 210d = 4$, $a + 6b + 42c + 336d = 12$, conſequently $a = -54$, $b = 47$, $c = -12$, $d = \frac{5}{6}$, therefore the ſum of the given ſeries is $\frac{-46}{3 \cdot 4 \cdot 5 \cdot 6 \cdot 7 \cdot 5} + \frac{47}{4 \cdot 5 \cdot 6 \cdot 7 \cdot 4} - \frac{12}{5 \cdot 6 \cdot 7 \cdot 3} + \frac{5}{6 \cdot 7 \cdot 2 \cdot 6} = \frac{61}{50400}$.

CASE 4. *To find the ſum of the infinite ſeries* $\frac{1}{1 \cdot 4 \cdot 7 \cdot 10} + \frac{9}{4 \cdot 7 \cdot 10 \cdot 13} + \frac{25}{7 \cdot 10 \cdot 13 \cdot 16} +$ &c.

Here $n = 1$, $m = 3$, $r = 3$, and the 3d differences are $= 0$; therefore $a + b + 4c = 1$, $a + 4b + 28c = 9$, $a + 7b + 70c = 25$, conſequently $a = \frac{1}{9}$, $b = \frac{-8}{9}$, $c = \frac{4}{9}$; ∴ the ſum of the given ſeries will be $\frac{1}{1 \cdot 4 \cdot 7 \cdot 3 \cdot 3 \cdot 9} - \frac{8}{4 \cdot 7 \cdot 3 \cdot 2 \cdot 9} + \frac{4}{7 \cdot 3 \cdot 9} = \frac{37}{2208}$.

CASE 5. *To find the Sum of the infinite Series* $\frac{1}{1\cdot3\cdot5\cdot7\cdot9}+\frac{4}{3\cdot5\cdot7\cdot9\cdot11}+\frac{10}{5\cdot7\cdot9\cdot11\cdot13}+\frac{20}{7\cdot9\cdot11\cdot13\cdot15}$.

In this caſe $n=1$, $m=2$, $r=4$, and the 4th differences become $=0$; therefore $a+b+3c+15d=1$, $a+3b+15c+105d=4$, $a+5b+35c+315d=10$, $a+7b+63c+693d=20$, conſequently $a=\frac{5}{16}$, $b=\frac{3}{16}$, $c=\frac{1}{16}$, $d=\frac{1}{48}$; and hence the ſum of the given ſeries becomes $\frac{5}{1\cdot3\cdot5\cdot7\cdot2\cdot4\cdot16}+\frac{3}{3\cdot5\cdot7\cdot2\cdot3\cdot16}+\frac{1}{5\cdot7\cdot2\cdot2\cdot16}+\frac{1}{7\cdot2\cdot48}=\frac{1}{384}$.

This propoſition may alſo be applied to find the ſum of all thoſe ſeries whoſe numerators being unity, the denominators ſhall be deficient by any number of correſponding terms, however taken: for as the product of all ſuch factors muſt form a progreſſion, whoſe differences will become equal to nothing, if ſuch products be aſſumed for the numerators of the given ſeries having its factors compleated, another ſeries will be formed equal to the given ſeries, whoſe ſum can be found by this propoſition.

CASE 1. *To find the ſum of the infinite ſeries* $\frac{1}{1\cdot2\cdot4\cdot6}+\frac{1}{2\cdot3\cdot5\cdot7}+\frac{1}{3\cdot4\cdot6\cdot8}+$&c.

By completing the factors in the denominators, and multiplying the numerators by the ſame quantities the given ſeries becomes $\frac{15}{1\cdot2\cdot3\cdot4\cdot5\cdot6}+\frac{24}{2\cdot3\cdot4\cdot5\cdot6\cdot7}+\frac{35}{3\cdot4\cdot5\cdot6\cdot7\cdot8}+$ &c. in which caſe $n=1$, $m=1$, $r=5$, and the 3d differences become

$=0$;

$=0$; therefore $a+b+2c=15$, $a+2b+6c=24$, $a+3b+12c=35$, conſequently $a=8$, $b=5$, $c=1$, and therefore the ſum of the ſeries required is $\frac{8}{1\cdot2\cdot3\cdot4\cdot5\cdot5}+\frac{5}{2\cdot3\cdot4\cdot5\cdot4}+\frac{1}{3\cdot4\cdot5\cdot3}=\frac{211}{7200}$.

CASE 2. *To find the ſum of the infinite ſeries* $\frac{1}{1\cdot5\cdot11}+\frac{1}{3\cdot7\cdot13}+\frac{1}{5\cdot9\cdot15}+$ &c.

This ſeries, when completed, becomes $\frac{189}{1\cdot3\cdot5\cdot7\cdot9\cdot11}+\frac{495}{3\cdot5\cdot7\cdot9\cdot11\cdot13}+\frac{1001}{5\cdot7\cdot9\cdot11\cdot13\cdot15}+\frac{1755}{7\cdot9\cdot11\cdot13\cdot15\cdot17}+$ &c. where $n=1$, $m=2$, $r=5$, and the 4th differences are $=0$; therefore $a+b+3c+15d=189$, $a+3b+15c+105d=495$, $a+5b+35c+315d=1001$, $a+7b+63c+693d=1755$, conſequently $a=96$, $b=48$, $c=10$, $d=1$; and hence the ſum of the given ſeries is $\frac{96}{1\cdot3\cdot5\cdot7\cdot9\cdot2\cdot5}+\frac{48}{3\cdot5\cdot7\cdot9\cdot2\cdot4}+\frac{10}{5\cdot7\cdot9\cdot2\cdot3}+\frac{1}{7\cdot9\cdot2\cdot2}=\frac{487}{18900}$.

CASE 3. *To find the ſum of the infinite ſeries* $\frac{1}{1\cdot4\cdot13\cdot16}+\frac{1}{4\cdot7\cdot16\cdot19}+\frac{1}{7\cdot10\cdot19\cdot22}+$ &c.

This ſeries reſolves itſelf into $\frac{70}{1\cdot4\cdot7\cdot10\cdot13\cdot16}+\frac{130}{4\cdot7\cdot10\cdot13\cdot16\cdot19}+\frac{208}{7\cdot10\cdot13\cdot16\cdot19\cdot22}+$ &c. where $n=1$, $m=3$, $r=5$, and the 3d differences $=0$; therefore $a+b+4c=70$, $a+4b+28c=130$, $a+7b+70c=208$, from whence $a=54$, $b=12$, $c=1$; therefore the ſum of the given ſeries is

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$$\frac{54}{1\cdot4\cdot7\cdot10\cdot13\cdot3\cdot5}+\frac{12}{4\cdot7\cdot10\cdot13\cdot3\cdot4}+\frac{[illegible]}{7\cdot10\cdot13\cdot3\cdot3}=\frac{227}{[illegible]}.$$

By this propoſition we may alſo inveſtigate the ſum of the ſeries when there are any number of deficient terms in the denominators, and where the laſt differences of the numerators become equal to nothing; for if the factors in the denominators be completed, and the numerators be multiplied by the ſame quantities, their differences will ſtill become equal to nothing.

CASE 1. *To find the ſum of the infinite ſeries* $\frac{1}{1\cdot3\cdot4\cdot6}+\frac{3}{2\cdot4\cdot5\cdot7}+\frac{6}{3\cdot5\cdot6\cdot8}+\frac{10}{4\cdot6\cdot7\cdot9}+\frac{15}{5\cdot7\cdot8\cdot10}+$&c.

This ſeries, by completing the factors in the denominators and multiplying the numerators by the ſame quantities, becomes $\frac{10}{1\cdot2\cdot3\cdot4\cdot5\cdot6}+\frac{54}{2\cdot3\cdot4\cdot5\cdot6\cdot7}+\frac{168}{3\cdot4\cdot5\cdot6\cdot7\cdot8}+$ &c. in which caſe $n=1$, $m=1$, $r=5$, and as the 5th differences are $=0$; $\therefore a+b+2c+6d+24e=10$, $a+2b+6c+24d+120e=54$, $a+3b+12c+60d+360e=168$, $a+4b+20c+120d+840e=400$, $a+5b+30c+210d+1680e=810$, from whence $a=0$, $b=0$, $c=-1$, $d=0$, $e=\frac{1}{2}$, conſequently the ſum of the given ſeries is $=-\frac{1}{3\cdot4\cdot5\cdot3}+\frac{1}{5\cdot2}=\frac{17}{180}$.

CASE 2. *To find the ſum of the infinite ſeries* $\frac{1}{1\cdot3\cdot7\cdot9}+\frac{5}{3\cdot5\cdot9\cdot11}+\frac{11}{5\cdot7\cdot11\cdot13}+\frac{19}{7\cdot9\cdot13\cdot15}+$&c.

By proceeding as before this ſeries becomes $\frac{5}{1\cdot3\cdot5\cdot7\cdot9}+$

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$\frac{35}{3\cdot5\cdot7\cdot9\cdot11}+\frac{99}{5\cdot7\cdot9\cdot11\cdot13}+$&c. where $n=1$, $m=2$, $r=4$, and the 4th differences $=0$; therefore $a+b+3c+15d=5$,

$$a+3b+15c+105d=35,\quad a+5d+35c+315d=99,$$

$a+7b+63c+693d=209$, conſequently $a=-1$, $b=\frac{3}{4}$, $c=\frac{1}{2}$, $d=\frac{1}{4}$, therefore the ſum of the given ſeries $=\frac{-1}{1\cdot3\cdot5\cdot7\cdot2\cdot4}+\frac{3}{3\cdot5\cdot7,2\cdot3\cdot4}+\frac{1}{5\cdot7\cdot2\cdot2\cdot2}+\frac{1}{7\cdot2\cdot4}=\frac{3}{140}$.

By a method ſimilar to that made uſe of in this propoſition may any number of factors be taken from the denominators of thoſe ſeries delivered in part the firſt, and alſo from a great variety of others; but as the examples here given muſt be ſufficient to point out the method of proceeding in all other caſes, we will proceed to the third part.

PART III.

THE ſum of every converging infinite ſeries, whoſe terms ultimately become equal to nothing, may always be exhibited by the ſum of another ſeries formed by collecting two or more terms of the former ſeries into one. This is not true; however, where the terms of the infinite ſeries continally diverge, or converge to any aſſignable quantity, and are affected with the ſigns +, −, alternately: for inſtance, the ſeries $\frac{1}{1}-\frac{1}{2}+\frac{1}{3}-\frac{1}{4}+$ &c. if we collect two terms into one, beginning at the firſt term, will become $-\frac{1}{2\cdot3}-\frac{1}{4\cdot5}-\frac{1}{6\cdot7}+$ &c. If we begin at the ſecond term it becomes $\frac{1}{1\cdot2}+\frac{1}{3\cdot4}+\frac{1}{5\cdot6}+$ &c.; neither of which gives the ſum of the aſſumed ſeries; but in this, and every other caſe of the like nature, a correction will be neceſſary: to determine the value of which, and from whence the neceſſity thereof ariſes, is the ſubject of this third part.

LEMMA.

If r be any quantity whatever, then will $\frac{1}{2r}=\frac{1}{r}-\frac{1}{r}+\frac{1}{r}-\frac{1}{r}+$ &c. *ad infinitum.*

For $\frac{1}{2r}=\frac{1}{r+r}=$ (by common diviſion) $\frac{1}{r}-\frac{1}{r}+\frac{1}{r}-\frac{1}{r}+$ &c. ad infinitum.

Cor. 1. Hence $-\frac{1}{2r}=-\frac{1}{r}+\frac{1}{r}-\frac{1}{r}+\frac{1}{r}-$ &c. ad infinitum.

Cor. 2. Hence alſo $\frac{z}{2v}=\frac{z}{v}-\frac{z}{v}+\frac{z}{v}-\frac{z}{v}+$ &c. ad infinitum;

and $-\frac{z}{2v}=-\frac{z}{v}+\frac{z}{v}-\frac{z}{v}+\frac{z}{v}-$ &c. ad infinitum.

PROP. I.

If $\frac{rn+m}{n}$ *be the general term of a ſeries formed by writing for n any ſeries of numbers in arithmetic progreſſion, and whoſe ſigns are alternately + and −; then if a ſeries be formed by col-*

lecting two terms into one, beginning at the firſt term, the ſum of the ſeries thence ariſing will be leſs than the ſum of the given ſeries by $\frac{1}{2r}$. *If a ſeries be formed by beginning at the ſecond Term, the ſum thereof will be greater than the ſum of the given ſeries by* $\frac{1}{2r}$.

For let $\frac{n}{rn+m}-\frac{n+a}{\overline{n+a}.r+m}$ be any two ſucceſſive terms of the ſeries, which, if we begin to collect at the firſt term (the firſt term being +) will be two terms to be collected into one, and which will therefore give $\frac{-am}{\overline{rn+m}\times\overline{n+a}.r+m}$ for a general term of the reſulting ſeries. Let us now make n infinite, and then the denominator of this term becomes infinite, and the numerator finite; therefore the terms of this latter ſeries at an infinite diſtance becoming infinitely ſmall, the ſeries will there terminate. Now, by making n infinite in the given ſeries, the two ſucceſſive general terms at an infinite diſtance become $\frac{1}{r}-\frac{1}{r}$; conſequently *this* ſeries is ſtill continued after the *other* terminates; and the terms of ſuch a continuation will be (as they begin with $\frac{n}{rn+m}-\frac{n+a}{\overline{n+a}.r+m}$ by making n infinite) $\frac{1}{r}-\frac{1}{r}+\frac{1}{r}-\frac{1}{r}+$ &c. which will alſo be continued ad infin. and whoſe ſum by the lemma is $\frac{1}{2r}$; conſequently the given ſeries exceeds that which is formed by collecting two terms into one, beginning at the firſt, by $\frac{1}{2r}$; hence the ſum of the *latter* ſeries $+\frac{1}{2r}$ will be equal to the ſum of the *former*. If we begin to collect at the ſecond term, then will $-\frac{n}{rn+m}+\frac{n+a}{\overline{n+a}\cdot r+m}$ be the two ſucceſſive general terms of the given ſeries to be collected into one; conſequently the continuation of the given ſeries when n becomes infinite will be $-\frac{1}{r}+\frac{1}{r}-\frac{1}{r}+\frac{1}{r}-$ &c. ad infinitum, whoſe ſum, by cor. 1. to the lem. is $-\frac{1}{2r}$; in this caſe, therefore, the ſum of the given ſeries is leſs than the ſum of the ſeries formed by collecting two terms into one, beginning at the ſecond term, by $\frac{1}{2r}$; hence the ſum of the *latter* ſeries $-\frac{1}{2r}$ will be equal to the ſum of the *former*.

CASE 1. *Let the given ſeries be* $\frac{1}{2}-\frac{2}{3}+\frac{3}{4}-\frac{4}{5}+$&c.

Here $r=1$, $n=1, 2, 3, 4,$ &c. and $m=1$. Now, if we begin to collect at the firſt term, the ſeries reſolves itſelf into $-\frac{1}{2\cdot3}-\frac{1}{4\cdot5}-\frac{1}{6\cdot7}-$ &c. and the correction, to be *added*, being $\frac{1}{2}$, we have $-\frac{1}{2\cdot3}-\frac{1}{4\cdot5}-\frac{1}{6\cdot7}-$&c. $+\frac{1}{2}$ for the ſum of the given ſeries. Now $-\frac{1}{2\cdot3}-\frac{1}{4\cdot5}-\frac{1}{6\cdot7}-$ &c. is well known to be equal to $-1+$ hyp. log. of 2; conſequently the ſum of the given ſeries is $=-\frac{1}{2}+$ hyp. log. of 2.

If we begin to collect at the ſecond term, the ſeries becomes $\frac{1}{1\cdot2}+\frac{1}{3\cdot4}+\frac{1}{5\cdot6}+$ &c. and the correction, to be *ſubtracted*, being $\frac{1}{2}$, we have $\frac{1}{1\cdot2}+\frac{1}{3\cdot4}+\frac{1}{5\cdot6}+$ &c. $-\frac{1}{2}$ for the ſum of the given ſeries; but $\frac{1}{1\cdot 2}+\frac{1}{3\cdot 4}+\frac{1}{5\cdot 6}+$ &c. is equal to the hyp. log. of 2; therefore the ſum of the given ſeries is $= -\frac{1}{2}+$ hyp. log. of 2, the ſame as before.

CASE 2. *Let the given ſeries be* $\frac{1}{3}-\frac{2}{5}+\frac{3}{7}+\frac{4}{9}+$ &c.

Here $r=2$, $n=1, 2, 3, 4,$ &c. $m=1$. Now, if we begin to collect at the ſecond term, the ſeries becomes $\frac{1}{1\cdot 3}+\frac{1}{5\cdot 7}+\frac{1}{9\cdot 11}+$ &c. and the correction, to be *ſubtracted*, being $\frac{1}{4}$, we have $\frac{1}{1\cdot 3}+\frac{1}{5\cdot 7}+\frac{1}{9\cdot 11}+$ &c. $-\frac{1}{4}$ for the ſum of the given ſeries; but $\frac{1}{1\cdot 3}+\frac{1}{5\cdot 7}+\frac{1}{9\cdot 11}+$ &c. is equal to a circular arc (A) of 22°½, whoſe radius is unity; therefore the ſum of the given ſeries $=A-\frac{1}{4}$.

PROP. II.

If $\frac{x+mz}{w+nv}$ *be the general term of a ſeries formed by writing for* n *any ſeries of numbers in arithmetic progreſſion, and whoſe terms are alternately* + *and* −; *then if a ſeries be formed by collecting two terms into one, beginning at the firſt term, the ſum of the ſeries thence ariſing will be leſs than the ſum of the given ſeries by* $\frac{z}{2v}$. *If a ſeries be formed by beginning at the ſecond term, the ſum thereof will be greater than the ſum of the given ſeries by* $\frac{z}{2v}$.

For let $\frac{x+nz}{w+nv} - \frac{x+\overline{n+a}.z}{w+\overline{n+a}.v}$ be any two ſucceſſive terms of the given ſeries, which, if we begin to collect at the firſt term, will be the two general terms to be collected into one, and will therefore give $\frac{axv-awz}{\overline{w+nv}\times\overline{w+\overline{n+a}.v}}$ for a general term of the reſulting ſeries. Let us now make n infinite and then this term will vaniſh, and conſequently the reſulting ſeries will terminate at an infinite diſtance. Now, by making n infinite in the given ſeries, the two ſucceſſive terms (as they begin with $\frac{x+nz}{w+nv} - \frac{x+\overline{n+a}.z}{w+\overline{n+a}.v}$ by making n infinite) become $\frac{z}{v} - \frac{z}{v}$; this ſeries, therefore, is ſtill continued after the other terminates; and the terms of ſuch a continuation will be $\frac{z}{v} - \frac{z}{v} + \frac{z}{v} - \frac{z}{v}$ + &c. ad infinitum, and whoſe ſum by cor. 2. to the lem. is $\frac{z}{2v}$; conſequently the given ſeries exceeds that which is formed by collecting two terms into one, beginning at the firſt, by $\frac{z}{2v}$; hence the ſum of the *latter* ſeries $+\frac{z}{2v}$ will be equal to the ſum of the *former*. Now, if we begin to collect at the ſecond term, then will $-\frac{x+nz}{w+nv} + \frac{x+\overline{n+a}.z}{w+\overline{n+a}.v}$ be two general terms of the given ſeries to be collected into one; conſequently the continuation of the given ſeries, when n becomes infinite, will be $-\frac{z}{v} + \frac{z}{v} - \frac{z}{v} + \frac{z}{v}$ – &c. ad infinitum, whoſe ſum by cor. 2. to the lem. is $-\frac{z}{2v}$; in this caſe, therefore, the ſum of the given ſeries is leſs than the ſum of the ſeries formed by collecting two terms into one, beginning at the ſecond term, by

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$\frac{z}{2v}$; hence the ſum of the *latter* ſeries $-\frac{z}{2v}$ will be equal to the ſum of the *former*.

CASE 1. *Let the given ſeries be* $\frac{7}{3}-\frac{11}{5}+\frac{15}{7}-\frac{19}{9}+$ &c.

Here $x=3$, $z=2$, $w=1$, $v=1$, $n=2, 4, 6, 8$, &c. Now, if we begin to collect at the firſt term, the ſeries becomes $\frac{2}{3\cdot5}+\frac{2}{7\cdot9}+\frac{2}{11\cdot13}+$ &c. and the correction, to be *added*, being 1, we have $\frac{2}{3\cdot5}+\frac{2}{7\cdot9}+\frac{2}{11\cdot13}+$ &c. $+1$ for the ſum of the given ſeries; but if $A=$ a circular arc of 45° whoſe radius is unity, it is well known that $\frac{2}{3\cdot5}+\frac{2}{7\cdot9}+\frac{2}{11\cdot13}+$ &c. $=1-A$; therefore the ſum of the given ſeries is $2-A$.

CASE 2. *Let the given ſeries be* $\frac{16}{1}-\frac{27}{2}+\frac{38}{3}-\frac{49}{4}+$ &c.

Here $w=1$, $v=1$, $x=16$, $z=11$, $n=0, 1, 2, 3$, &c. Now, if we begin to collect at the firſt term, the ſeries becomes $\frac{5}{1\cdot2}+\frac{5}{3\cdot4}+\frac{5}{5\cdot6}+$ &c. and the correction, to be *added*, being $\frac{11}{2}$, we have $\frac{5}{1\cdot2}+\frac{5}{3\cdot4}+\frac{5}{5\cdot6}+$ &c. $+\frac{11}{2}$ for the ſum of the given ſeries; but $\frac{5}{1\cdot2}+\frac{5}{3\cdot4}+\frac{5}{6\cdot7}+$ &c. is equal to $5\times$ hyp. log. of 2, conſequently the ſum of the given ſeries is equal to $\frac{11}{2}+5\times$ hyp. log. of 2.

Becauſe $\frac{axv-awx}{\overline{w+nv}\times\overline{w+n+a}\cdot v}$, the general term of the ſeries formed by reducing two terms into one, has its numerator independent of the value of, *n*, it is manifeſt, that the numerators of that ſeries will be all equal. Now, if a ſeries be aſſumed, the numerators of whoſe terms are unity, and in every other reſpect the ſame as the ſeries in this propoſition, that is, if $\frac{1}{w+nv} - \frac{1}{w+\overline{n+a}.v}$ be two ſucceſſive terms of a ſeries, it is manifeſt, that if every two terms of this ſeries be reduced into one, the general term of the reſulting ſeries will be $\frac{-av}{\overline{w+nv} \times \overline{w+\overline{n+a}.v}}$, where the numerator is a conſtant quantity $-av$; conſequently the ſum of the ſeries whoſe general term is $\frac{avx-awz}{\overline{w+nv} \times \overline{w+\overline{n+a}.v}}$ is to the ſum of the ſeries whoſe general term is $\frac{-av}{\overline{w+nv} \times \overline{w+\overline{n+a}.v}}$ as $vx - wx$ to $-v$, or in a given ratio; whenever, therefore, the ſum of the latter ſeries can be found, the ſum of the former can be found, and conſequently, after proper correction, the ſum of the ſeries in this propoſition can be found.

Hence, therefore, in the two caſes given above in whatever arithmetic progreſſion the numerators may proceed, the ſum of the former can always be expreſſed by circular arcs, and the latter by the hyp. log. of 2.

Hence alſo, as it appears from lem. 1. part the firſt, that the ſum of the ſeries $\frac{1}{1} - \frac{1}{r+1} + \frac{1}{2r+1} - \frac{1}{3r+1} +$ &c. can always be expreſſed by circular arcs and logarithms, it is manifeſt, that if the numerators form any arithmetic progreſſion, the ſum of ſuch ſeries may be found by this propoſition, and will always be exhibited by circular arcs and logarithms.

Beſides the ſeries contained in the foregoing propoſitions, a great variety of other ſeries might be produced where a correction is neceſſary, after collecting two terms into one, in order to exhibit the true value of the given ſeries. As the proper correction, however, may always be found from the principles delivered in the above propoſitions, that is, by conſidering what the terms of the given ſeries become at an infinite diſtance, I ſhall only add one or two inſtances more, and conclude what I at preſent intend to offer on this ſubject.

EX. 1. *Let it be required to find the ſum of the infinite ſeries* $\frac{3 \cdot 4}{1 \cdot 2} - \frac{4 \cdot 5}{2 \cdot 3} + \frac{5 \cdot 6}{3 \cdot 4} - \frac{6 \cdot 7}{4 \cdot 5} + \&c.$

This, by reſolving two terms into one, becomes $\frac{16}{1 \cdot 2 \cdot 3} + \frac{24}{3 \cdot 4 \cdot 5} + \frac{32}{5 \cdot 6 \cdot 7} - \&c.$; and as the terms of the given ſeries continually approach to unity, the correction, to be *added*, is $\frac{1}{2}$, conſequently $\frac{16}{1 \cdot 2 \cdot 3} + \frac{24}{3 \cdot 4 \cdot 5} + \frac{32}{5 \cdot 6 \cdot 7} - \&c. + \frac{1}{2}$ is equal to the ſum of the given ſeries; but by prop. 1. part I. the ſum of the ſeries $\frac{16}{1 \cdot 2 \cdot 3} + \frac{24}{3 \cdot 4 \cdot 5} + \frac{32}{5 \cdot 6 \cdot 7} + \&c.$ is equal to $8S - 2$ (S being the hyp. log. 2.) conſequently the ſum of the given ſeries is $8S - 1\frac{1}{2}$.

EX. 2. *Let it be required to find the ſum of the infinite ſeries* $\frac{1 \cdot 2}{1 \cdot 3} - \frac{2 \cdot 3}{3 \cdot 5} + \frac{3 \cdot 4}{5 \cdot 7} - \frac{4 \cdot 5}{7 \cdot 9} + \&c.$

This ſeries, by reſolving two terms into one, becomes $\frac{4}{1 \cdot 3 \cdot 5} + \frac{8}{5 \cdot 7 \cdot 9} + \frac{12}{9 \cdot 11 \cdot 13} + \&c.$ and as the terms of the given ſeries continually approach to $\frac{1}{4}$, the correction, to be *added*, will be $\frac{1}{8}$, therefore $\frac{4}{1\cdot3\cdot5}+\frac{8}{5\cdot7\cdot9}+\frac{12}{9\cdot11\cdot13}+\&c.+\frac{1}{8}$ is = to the ſum of the given ſeries; but by prop. 1. part I. the ſum of $\frac{4}{1\cdot3\cdot5}+\frac{8}{5\cdot7\cdot9}+\frac{12}{9\cdot11\cdot13}+\&c.$ is equal to $\frac{1}{4}S+\frac{1}{8}$ (S being a circular arc of 45°, whoſe radius is unity) hence the ſum of the given ſeries is $\frac{1}{4}S+\frac{1}{4}$.

This method is not only applicable to thoſe caſes, where the given ſeries reſolves itſelf into another, whoſe ſum is either accurately known or can be expreſſed by circular arcs and logarithms, but alſo to thoſe caſes where we want to approximate to the value of the given ſeries, as it muſt, in general, be neceſſary firſt to render the terms of the ſeries converging, by collecting two into one, before the operation of approximation begins, and conſequently a correction of this latter is neceſſary in order to exhibit the value of the given ſeries.

XXVI. *A new Method of finding the equal Roots of an Equation, by Diviſion. By the Rev.* John Hellins, *Curate of* Conſtantine, *in* Cornwall; *communicated by* Nevil Maſkelyne, *D. D. F. R. S. and Aſtronomer Royal.*

Read June 20, 1782.

THE following theorems are a production of juvenile years. They were invented about twelve years ago, when algebra was my favourite ſtudy; and one of them (the firſt) was publiſhed as a ſpecimen of this method of extracting the equal roots of an equation about ten years ago. Since that time my avocations have left me but very little leiſure for improving any invention of this kind. Theſe theorems, then, are in their crude ſtate; however, ſuch as they are, I flatter myſelf, they will afford an eaſier ſolution of equations that have equal roots than is generally known, and be acceptable to the ingenious algebraiſt.

THEOREM I.

If the cubic equation $x^3 - px^2 + qx - r = 0$ *has two equal roots, each of them will be* $(x) = \frac{pq - 9r}{2pp - 6q}$.

DEMONSTRATION.

Call the three roots a, a, and b; then, by the compoſition of equations we ſhall have $x^3 \left.\begin{matrix} -2a \\ -b \end{matrix}\right\} x^2 \left.\begin{matrix} +aa \\ +2ab \end{matrix}\right\} x - aab = 0$, where

$2a+b=p$, $aa+2ab=q$, and $aab=r$; which values being written in our theorem, we have $x\ \left(=\frac{pq-9r}{2pp-6q}\right)=\frac{2aaa+4aab+aab+2abb-9aab}{8aa+8ab+2bb-6aa-12ab}=\frac{2aaa-4aab+2abb}{2aa-4ab+2bb}=a$. Q. E. D.

EXAMPLE I.

If the equation $x^3+5x^2-32x+36=0$ has two equal roots, it is propoſed to find them by the above theorem.

Here $p=-5$, $q=-32$, and $r=-36$; theſe values being written in the theorem, we have $\frac{-5\times-32-9\times-36}{2\times25-6\times-32}=\frac{160+324}{50+192}=\frac{484}{242}=2$, which being written for x, the equation becomes $8+20-64+36$, which is evidently $=0$; conſequently 2 and 2 are roots of it.

Otherwiſe, 2, the value of x given by the theorem, being written for it in the quadratic equation $3x^2+10x-32=0$, the reſult is $12+20-32=0$.

Or, dividing the given cubic by the quadratic $\overline{x-2}|^2$, we have $x^2-4x+4)\,x^3+5x^2-32x+36\,(x+9$; therefore the three roots are 2, 2, and -9.

EXAMPLE II.

Given $x^3+\frac{10}{7}x^2-\frac{4000}{9261}=0$, an equation which has equal roots, to find them.

Here $q=0$, and the theorem gives $\frac{-36000\times49}{200\times9261}=\frac{-20}{21}$, which value being written for x the equation vaniſhes.

THEOREM II.

If the biquadratic equation $x^4-px^3+qx^2-rx+s=0$ *has two equal roots, make* $A=\frac{12r-2pq}{3pp-8q}$, $B=\frac{pr-16s}{3pp-8q}$, $C=\frac{4B-2q}{4A+3p}$, *and* $D=\frac{r}{4A+3p}$, *and you will have* $x=\frac{D-B}{A-C}$.

A ſynthetical demonſtration of this theorem would be very long: the INVESTIGATION is as follows.

It has been demonſtrated by the writers on algebra, that, if a biquadratic equation, as $x^4-px^3+qx^2-rx+s=0$, has two equal roots, one of them may be had from the equation $4x^3-3px^2+2qx-r=0$. Multiply this equation by x, and the original one by 4, and take the difference of the two, which will be $px^3-2qx^2+3rx-4s=0$. Again, if this equation be multiplied by 4, and the other cubic by p, and their difference taken, we ſhall have $\overline{3pp-8q}\times x^2+\overline{12r-2pq}\times x+pr-16s=0$, or $x^2+\frac{12r-2pq}{3pp-8q}x+\frac{pr-16s}{3pp-8q}=0$, or $x^2+Ax+B=0$, putting A and B for the known quantities in the ſecond and third terms. Now multiply this equation by $4x$, and take the firſt cubic from it, and we ſhall have $\overline{4A+3p}\times x^2+\overline{4B-2q}\times x+r=0$, which being divided by $4A+3p$, and C and D put equal to $\frac{4B-2q}{4A+3p}$ and $\frac{r}{4A+3p}$ reſpectively, gives $x^2+Cx+D=0$; and this equation being taken from the other quadratic, there remains $\overline{A-C}\times x+B-D=0$; conſequently $x=\frac{D-B}{A-C}$. *Q. E. I.*

COROLLARY I. From the above inveſtigation it appears, that one of the equal roots may alſo be obtained from either of theſe two quadratic equations, of which the firſt ſeems moſt eligible,

as

as the co-efficients of it are leſs complex than thoſe of the other:

$$\overline{3pp-8q} \times x^2 + \overline{12r-2pq} \times x + pr - 16s = 0,$$

and $\overline{4A+3p} \times x^2 + \overline{4B-2q} \times x + r = 0$. And theſe, when $p=0$, become $-8qx^2+12rx-16s=0$,

$$\text{and } -\frac{48r}{8q}x^2 + \overline{\frac{64s}{8q}-2q} \times x + r = 0,$$

$$\text{or } x^2 - \frac{3r}{2q}x + \frac{2s}{q} = 0,$$

$$\text{and } x^2 + \frac{qq-4s}{3r}x - \frac{q}{6} = 0.$$

COROL. 2. If both p and q vaniſh, then, from either of the quadratics we get $x = \frac{4s}{3r}$, perfectly agreeing with the cubic $px^3 - 2qx^2 + 3rx - 4s = 0$, which, when p and q vaniſh, becomes $3rx - 4s = 0$. And this equation is of uſe; becauſe, in this caſe, the theorem fails, one of the diviſors being $=0$.

COROL. 3. From the equation $4x^3 - 3px^2 + 2qx - r = 0$, which, when p and q vaniſh, becomes $4x^3 - r = 0$, we alſo get $x = \sqrt[3]{\frac{r}{4}}$, another expreſſion of the ſame value of x.

COROL. 4. When $r=0$, $D=0$, and from the equation $x^2+Cx+D=0$, we have $x=-C$.

EXAMPLE I.

If the equation $x^4 * - 9x^2 + 4x + 12 = 0$ has equal roots, it is propoſed to find them.

Here $p=0$, $q=-9$, $r=-4$, and $s=12$; and

$$\text{A becomes} = \frac{12\times-4}{-8\times-9} = \frac{-2}{3},$$

$$\text{B} \;.\;.\;. = \frac{-16\times-12}{-8\times-9} = \frac{-8}{3},$$

$$\text{C} \;.\;.\;. = \frac{4\times\frac{-8}{3}+18}{4\times\frac{-2}{3}} = \frac{-11}{4},$$

$$\text{D} \;.\;. = \frac{-4}{\frac{-8}{3}} = \frac{3}{2},$$

$$\text{and } \frac{\text{D}-\text{B}}{\text{A}-\text{C}} = \frac{\frac{3}{2}+\frac{8}{3}}{\frac{-2}{3}+\frac{11}{4}} = \frac{18+32}{-8+33} = \frac{50}{25} = 2,$$

which being written for x, the equation becomes $16-36+8+12=0$; therefore 2 is one of the roots.

The ſame value of x may be diſcovered from either of the quadratic equations mentioned in corollary 1. The proper values of the co-efficients being written in the firſt of them, it becomes $x^2-\frac{2}{3}x-\frac{8}{3}=0$, where one value of x is $\frac{1+\sqrt{25}}{3}=2$. The other quadratic becomes $x^2-\frac{11}{4}x+\frac{3}{2}=0$, one of whoſe roots is $\frac{11+\sqrt{25}}{8}=2$.

EXAMPLE II.

It being known that the equation $x^4-x^3-7x^2+13x-6=0$ has two equal roots, to find them.

Here $p=1$, $q=-7$, $r=-13$, and $s=-6$; and $\text{A}=\frac{-142}{59}$, $\text{B}=\frac{83}{59}$, $\text{C}=\frac{-1158}{391}$, $\text{D}=\frac{767}{391}$, $\text{D}-\text{B}=\frac{12800}{23009}$, $\text{A}-\text{C}=\frac{12800}{23069}$, and $\frac{\text{D}-\text{B}}{\text{A}-\text{C}}=\frac{12800}{12800}=1$, one of the roots ſought.

The ſame value of x may be found from either of the two general quadratic equations given in corollary 1. From the firſt of them we get one value of $x = \frac{71 - \sqrt{144}}{59} = 1$. And from the other, one value $= \frac{579 - \sqrt{35344}}{39}$, which is alſo $= 1$.

EXAMPLE III.

Given the equation $x^4 - \frac{1}{2}x + \frac{3}{16} = 0$, in which two values of x are equal to each other, to find them.

By corollary 2. we have $x = \frac{4 \times 3}{16} \div \frac{3 \times 1}{2} = \frac{8}{16} = \frac{1}{2}$. By corol. 3. x is $= \sqrt[3]{\frac{1}{8}} = \frac{1}{2}$.

THEOREM III.

If the ſurſolid equation $x^5 - px^4 + qx^3 - rx^2 + sx - t = 0$ *has two roots equal to each other, and you make* $A = \frac{15r - 3pq}{4pp - 10q}$, $B = \frac{2pr - 20s}{4pp - 10q}$, $C = \frac{25t - ps}{4pp - 10q}$, $D = \frac{5B - 3q}{5A + 4p}$, $E = \frac{5C + 2r}{5A + 4p}$, $F = \frac{s}{5A + 4p}$, $G = \frac{B - E}{A - D}$, $H = \frac{F + C}{A - D}$, $I = \frac{B - H}{A - G}$, *and* $K = \frac{C}{A - G}$, *then ſhall one of the equal values of* x *be* $= \frac{H - K}{I - G}$.

The inveſtigation of this theorem being altogether ſimilar to that of the laſt, it is unneceſſary to give it here.

The difference of equations being taken as in the inveſtigation of theorem II. it will appear, that one of the equal roots may alſo be had from any one of the following five equations, of which ſometimes one, ſometimes another, will be the moſt eligible.

1. $5x^4-4px^3+3qx^2-2rx+s=0.$
2. $px^4-2qx^3+3rx^2-4sx+5t=0.$
3. $x^3+Ax^2+Bx+C=0.$
4. $x^3+Dx^2+Ex-F=0.$
5. $x^2+Gx+H=0.$

It is obvious, that, when p vaniſhes, the work will be conſiderably ſhortened; and when both p and q are wanting, though the above *formula* fails, yet the equal root may be eaſily obtained from the equation $px^4-2qx^3+3rx^2-4sx+5t=0$, which in that caſe becomes $3rx^2-4sx+5t=0$. Whenever s is wanting, F, in the ſecond cubic above, will be $=0$; and conſequently x may be found from the quadratic equation $x^2+Dx+E=0$. But in any of theſe caſes the equal root may be found by diviſion. However, the operation probably will not, in general, be ſo ſhort as extracting the root of the quadratic; I will therefore haſten to give an example or two of the uſe of the theorem.

EXAMPLE I.

Given $x^5+x^3-x^2+0{\cdot}09433=0$, to find x, two values of it being equal to each other.

Here $p=0$, $q=1$, $r=1$, $s=0$, $t=-0{\cdot}09433$, and we get

$A=-1{\cdot}5$ $\qquad$ $F=0$

$B=0$ $\qquad$ $G=-0{\cdot}2231$

$C=+0{\cdot}2358$ $\qquad$ $H=-0{\cdot}1241$

$D=+0{\cdot}4$ $\qquad$ $I=-0{\cdot}0972$

$E=-0{\cdot}4238$ $\qquad$ $K=-0{\cdot}185$

$$\text{and } x=\frac{H-K}{I-G}=0.48.$$

The proper values of the co-efficients being written in the five equations before mentioned, and ſome of them divided by the co-efficient of the higheſt power of x, we have theſe four equations, in each of which one value of x is one of the equal ones ſought:

$$x^3 * + 0{\cdot}6x - 0{\cdot}4 = 0.$$
$$x^3 - 1{\cdot}5x^2 * + 0{\cdot}2358 = 0.$$
$$x^2 + 0{\cdot}4x - 0{\cdot}4238 = 0.$$
$$x^2 - 0{\cdot}2231x - 0{\cdot}1241 = 0.$$

Now the moſt eligible equation is the quadratic $x^2 + 0{\cdot}4x - 0{\cdot}4238 = 0$, whoſe affirmative root is $\sqrt{0{\cdot}4638} - 0{\cdot}2 = 0{\cdot}4811$, agreeing with the value of x found above, but true to two places lower in the decimal.

EXAMPLE II.

To find the two equal values of x in the equation $64x^5 - 20x^2 + 3 = 0$.

The given equation being divided by 64, we have $x^5 - 0{\cdot}3125x^2 + 0{\cdot}046875 = 0$; and then, from the firſt of the five equations given above, we get $5x^4 - 0{\cdot}625x = 0$, and $x = \sqrt[3]{0{\cdot}125} = 0{\cdot}5$. But from the ſecond of the equations juſt mentioned, we have $0{\cdot}9375x^2 - 0{\cdot}234375 = 0$, or $x^2 = \frac{0{\cdot}234375}{0{\cdot}9375} = 0{\cdot}25$, and $x = \sqrt{0{\cdot}25} = 0{\cdot}5$.

From the foregoing few pages it is evident, that rules may be made for finding the equal roots of equations of more than five dimenſions by diviſion; but the operations by them will, in moſt caſes, be long and tedious. It is obvious, however, that ſuch equations may be depreſſed to any dimenſion the algebraiſt pleaſes.

It has indeed been ſuppoſed, that the number of equations that have equal roots is but ſmall, and, conſequently, that the chief uſe of the rules for finding their roots is to get limits and approximations to the roots of equations in general. That uſe, it muſt be allowed, were it the only one, is ſufficient to pay for inveſtigating them. But if the equations that have equal roots ſhould hereafter be found not ſo few as has been generally received, then the uſe of the above theorems will become more extenſive.

I beg leave to add, that this ſhort eſſay is but a ſmall part of a work, in which, if I ſhould ever have leiſure to put a finiſhing hand to it, ſomething more on this ſubject may very probably appear. In the mean while, I hope, this little piece will be candidly received by thoſe who have more leiſure and better abilities for ſtudies of this kind.

Conſtantine,
February 9, 1782.

XXVII. *Some farther Conſiderations on the Influence of the Vegetable Kingdom on the Animal Creation. By* John Ingen-houſz, *Counſellor to the Court, and Body Phyſician to the Emperor, F. R. S. &c.*

Read June 13, 1782.

UPON being informed, a few months ago, as well by private letters as from the Critical Review, that my doctrine was quite over-turned by the fifth volume of Dr. PRIESTLEY, and by an experiment quoted in the book of Mr. CAVALLO on Air; I invited ſome of my friends here to aſſiſt at ſome deciſive experiments, of which I will here give an exact account. I told them the whole reſult which was to be expected from them, if my ſyſtem was founded on nature, explaining to them before-hand the theory of theſe reſults, and promiſing, at the ſame time, that, if the reſult ſhould fail, I ſhould myſelf be the firſt to diſcredit my own ſyſtem. I had the ſatisfaction to convince them that the reſult did fully anſwer my prediction and expectation. Theſe experiments are the following, all made in a hot-houſe of the Botanical Garden in the winter of 1782.

I expoſed to the ſun-ſhine ſix globular glaſs veſſels, each containing about 160 cubic inches of ſpace, all filled with pump-water, which was boiled during more than two hours, and poured quite hot into the glaſs veſſels, on purpoſe to prevent any acceſs of air to the water.

Exp. I. In two of theſe veſſels I put as much of the *conferva rivularis* (a water plant, claſſed by LINNÆUS amongſt the *cryptogamia*) as was ſufficient to take up the ſpace of about an inch ſquare.

Exp. II. In the two next veſſels I ſuſpended by threads tied to bits of cork, ſome pieces of woollen and ſilk cloth of different colours, as white, ſcarlet, green, and brown, having previouſly wetted them in ſome boiled water, on purpoſe to free them from all air.

Exp. III. In the two remaining veſſels I placed nothing at all.

Exp. IV. In another veſſel of the ſame form and ſize I put ſome of the *conferva rivularis*, and filled it with pump water.

All theſe globular veſſels were inverted, with their orifices immerſed in veſſels filled with quickſilver, for the purpoſe of preventing effectually any communication between the contents of the veſſels and the atmoſphere.

Reſult of experiment I. The firſt two days neither of the veſſels contained any air, and even the ſmall quantity of air, which here and there adhered in the form of a bubble to the fibres of the vegetable when it was ſhut up in the veſſel, had entirely diſappeared. The third day, in the morning, ſome air bubbles began to riſe from every part of the *conferva* in both glaſſes; and in the afternoon of the ſame day, a great quantity of air bubbles roſe continually from it. I took at that time the vegetable out of one of theſe veſſels. I plunged a wax taper, juſt extinguiſhed, into the orifice of this veſſel, on purpoſe to ſee whether the air, already extricated from the *conferva*, was dephlogiſticated or not. The wax taper took flame immediately with an uncommon ſplendour. After this I poured the half of the water from the globular veſſel into a common bottle, and corked it. I inverted this bottle afterwards in an earthen veſſel filled with boiled water. I placed this apparatus near the fire till the water in the bottle began to boil; after which I cooled the whole, and found a good quantity of air collected in the bottle, which air proved to be dephlogiſticated. When I drew the vegetable out of the glaſs veſſel I obſerved the water to ſparkle almoſt like Seltzer water, or like water impregnated by art with fixed air. The vegetable which was ſtill kept in the ſecond bottle of exp. I. continued to yield air in the ſun-ſhine, till it ceaſed to throw up any more air, towards the ſeventh or eighth day of its being ſhut up in the veſſels. When, after this time, this globular veſſel was ſhook, the water became full of ſmall air bubbles, which for the moſt part roſe to the inverted bottom of the veſſel, great part of them ſettling upon the vegetable, which appeared all covered with them. This ſparkling air, which became viſible by ſhaking the glaſs, could not but be air originally produced by the *conferva*, and ſo looſely joined with the water, that it diſengaged itſelf in a great meaſure from it by the motion of the veſſel. After the tenth day the vegetable began to appear withered, grew yellow, and began to die. I found about eight cubic inches of dephlogiſticated air collected in the veſſel. This proved to be of a very eminent quality, its goodneſs being of 352°; that is to ſay, that from a mixture of one meaſure of this air, and as many meaſures of nitrous air as were neceſſary to complete the full ſaturation, there were deſtroyed three meaſures and $\frac{52}{100}$ of a meaſure, the teſt being made with Abbé FONTANA's Eudiometer, employed in the manner deſcribed in my book upon Vegetables, p. 278. *et ſeq.* The quality of this air was ſuperior to that of any air I ever got from this plant in freſh pump water, its goodneſs proving, in general, to be from 260 to 330°, in the hot-houſe; this was during the winter, for I never had been able to obtain ſuch fine air from this vegetable in the ſummer [*]; the reaſon of which I will explain elſewhere.

Theory of exp. I. Boiled water, having loſt its air, is very much diſpoſed to imbibe it from all bodies which contain this fluid; and therefore, during the firſt day or two of its expoſure to the ſun-ſhine with the vegetable, this water abſorbed all the air which the plant emitted; and even that which had remained entangled between the fibres of the vegetable when it was immerſed in this water. The water being at laſt ſaturated with this air could take up no more; and therefore, whatever air, after this ſaturation, came forth from the vegetable roſe to the top of the veſſel. The quantity of this dephlogiſticated air was ſmaller than that which an equal bulk of the ſame plant commonly yields in freſh pump water, becauſe a great deal of air was at firſt taken in or abſorbed by the boiled water; which abſorption does not happen, or at leaſt is not ſo great, when pump water is uſed, as this water is always nearly ſaturated with air. The air, thus obtained in boiled water, was of a finer quality than that commonly obtained by the ſame means in pump water; becauſe this air, being entirely free of air diſengaged from freſh water, muſt of courſe be ſo much the purer dephlogiſticated air. The water of the firſt veſſel, and which was taken out of it as ſoon as air bubbles began to riſe from the plant, ſparkled like Seltzer water, and yielded by heat dephlogiſticated air, becauſe it was then already ſaturated with dephlogiſticated air iſſuing from the vegetable. The water of this veſſel being ſhook, after the vegetable ceaſed to throw up any more air in a viſible way, ſtill continued to ſparkle; becauſe, though the vegetable by loſing gradually its vigour, was at laſt no more able to throw up air in viſible bubbles, yet it had ſtill enough of its vital power left to keep the water ſaturated with dephlogiſticated air, ſo as to ſparkle when ſhook. This vegetable, continually robbing the water of its natural air, found at laſt nothing more in it to ſupport its life, and therefore at laſt languiſhed and periſhed; which it did ſo much the ſooner, from the contact of the dephlogiſticated air, with which the water was impregnated, and to which the vegetable had been all that time expoſed, having hurt its conſtitution (it is well known, that plants die in dephlogiſticated air) and thus haſtened its death. The ſparkling quality of the water did not ceaſe entirely till the vegetable was quite deprived of its life. The water began to ſparkle every day very briſkly, by being ſhook after the apparatus had been expoſed an hour or two to the ſun-ſhine, during the time the vegetable was in its full vigour, and ceaſed to do ſo ſome time after ſun-ſet, or after the apparatus had been withdrawn from the ſun's light; becauſe this vegetable, like all others, elaborates no dephlogiſticated air but by the influence of the ſun; and becauſe this air, being but looſely united with the water, diſengaged itſelf again from it, and roſe to the inverted bottom of the veſſel. The water being thus deprived of the dephlogiſticated air, ceaſed to ſparkle any more, till it became again ſaturated with it, after the apparatus had been expoſed again, during ſome time, to the influence of the light.

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[*] By continuing to make experiments, during the whole winter, in the hot-houſes of the botanical garden, I found that the *conferva rivularis* yielded dephlogiſticated air of a much ſuperior quality to that I had ever been able to get from it in the ſummer, in the open air; whereas thoſe plants, ſuch as the *agave Americana, cactus triangularis*, &c. which yielded in the ſummer the beſt air, did ſcarce yield any in the winter (and that of a quality ſcarce better than common air) though placed next to the *conferva*. The quantity of dephlogiſticated air I got in the winter from the *conferva* was ſo great, that as much of this vegetable as occupies about the ſpace of one cubic inch commonly yielded from 12 to 16 cubic inches of this air in the ſpace of three or four days, when the ſun did ſhine, the quantity of pump-water being about 160 cubic inches. The green matter which Dr. PRIESTLEY mentions as ſpontaneouſly produced from pump-water, gave in the winter alſo a tolerable quantity of dephlogiſticated air, of a good quality, though not ſo fine, or in ſuch large quantities, as it is uſed to give in the ſummer. It ſeems to be a general rule, that the greater the quantity of air obtained from vegetables in the ſun is, the better is its degree of goodneſs.

Reſult of exp. II. No air at all was produced in the veſſel containing the pieces of cloth, during three weeks expoſure to the ſun-ſhine.

Theory of exp. II. Boiled water, having loſt its air, could yield none, at leaſt till after a long time, when ſome degree of corruption took place in the animal ſubſtance, *viz.* the pieces of cloth.

Reſult of exp. III. Not an atom of air has appeared in this veſſel, though it ſtood about two months upon the ſame place.

Theory of exp. III. Boiled water having no air, the ſun could extricate none from it.

Reſult of exp. IV. the *conferva* began to yield air bubbles the very ſame day, a little while after its expoſure to the ſun. The next day it threw up an immenſe quantity of them. The fifth day it begun to throw up leſs, and ceaſed entirely about the ſeventh day, when the quantity of about fourteen cubic inches of dephlogiſticated air, of an excellent quality, though leſs fine than that obtained in exp. I. was collected. The water ſparkled, as does Seltzer water, by the veſſel being ſhook. This water being expoſed to the fire, in an inverted veſſel, yielded a good quantity of air, which was ſo far dephlogiſticated as to be able to kindle a wax taper juſt extinguiſhed. After the tenth day the vegetable began to die.

Theory of exp. IV. The vegetable threw up very ſoon air bubbles, becauſe this water, being in its natural ſtate, and thus ſaturated with air, could not abſorb much of the air iſſuing from the vegetable, which air muſt, of courſe, ſoon riſe up in viſible bubbles. A great deal more air was collected than in exp. I. becauſe leſs of the air iſſuing from the plant was abſorbed by this water than in exp. I. The air obtained was not ſo good as that obtained in exp. I.; becauſe the air in this experiment was ſomewhat infected by the air iſſuing from the water, which was but common air. The water ſparkled when the veſſel was ſhook, becauſe this water, though it had probably loſt ſome of its own air, yet had aſſumed a great deal of air from the plant *, which air diſengages itſelf from the water very eaſily, juſt as fixed air does; the more ſo when the water is moved.

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* It ſeems, that dephlogiſticated air has by no means ſuch a ſtrong attraction to water, in other words, has not ſo much affinity with it, as common air has: it appears to be but looſely united with water, and quits it very eaſily by the water being ſhaken, or even though the water be kept quiet. May not this quality be looked upon as a providential one? for thus the dephlogiſticated air, produced by water plants, is continually emitted by the water, and diffuſed through the common ſtock; and for this reaſon the water, when ſhook during the day-time, always ſparkled as champaigne; becauſe at that time the water was always kept ſaturated by the dephlogiſticated air, iſſuing continually out of the vegetable. But the water ceaſed to ſparkle, after having been withdrawn from the ſun-ſhine during ſome hours; becauſe the dephlogiſticated air, with which the water was ſaturated during the day-time (ſuppoſing the apparatus to be kept expoſed to the ſun) being but looſely united with the water, diſengaged itſelf gradually from it, and roſe to the top of the veſſel. The ſparkling quality of this water returned after the veſſel had been expoſed afreſh to the ſun; becauſe the vegetable, reſuming in that expoſition its daily operation, communicated a freſh ſupply of dephlogiſticated air to the water. This ſparkling quality returned every day, as long as there remained any life in the vegetable; after which the water ſparkled no more (though ſhook ever ſo much) either by day or in the night.

This water yielded, by heat, true dephlogiſticated air; whereas the ſame water, when it has not been expoſed to the action of a vegetable, yields by heat nothing but common air. The reaſon of it is, that the air elaborated by the plant, with which this water was ſaturated, was real dephlogiſticated air. The vegetable at laſt languiſhed and began to die, becauſe the water was impregnated with dephlogiſticated air, which being an excrement of the plant is hurtful to its conſtitution. Beſides, this water had at laſt loſt the moſt part of, or perhaps all, its own ſtock of common air; and with this all the nutritive nouriſhing and phlogiſtic particles, which were taken in by the plant, and was therefore become leſs fit to keep up vegetable life.

All theſe experiments were repeated frequently, and always with the ſame general reſults *.

I think the abovementioned facts will be looked upon as quite ſufficient to put my doctrine out of all farther queſtion. I have many more facts, perhaps equally demonſtrative with thoſe juſt deſcribed; but, as this paper is already too long, I will keep them for ſome other opportunity. However, I cannot forbear making ſome farther remarks by which the point in queſtion may be ſtill farther illuſtrated.

If it was the water, and not the vegetable, which yielded the dephlogiſticated air; and thus, if the reaſon why water plants and the green matter ceaſe at laſt to throw up more air (if the

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* That air is thrown out of living vegetables expoſed to the ſun-ſhine, was already obſerved by the rev. Dr. HALES, as may be ſeen in vol. I. of his Statical Eſſays, p. 110. The apparatus which he uſed for this experiment is repreſented by fig. XVII. plate VII. But this ineſtimable philoſopher, not even ſuſpecting that this air was of a peculiar nature, did not collect it. BOYLE obtained much air from vegetables *in vacuo*.

water is not renewed) is, that the water being at laſt exhauſted of its air can yield no more, it would neceſſarily follow, that the water, thus ſuppoſed to be deprived of air, ſhould be ſimilar to diſtilled or boiled water; but it is quite the reverſe. This very water, inſtead of being exhauſted of air, gives evident ſigns of being over ſaturated with it. It ſparkles almoſt as champaign does, when ſhook by day-time in the ſun; and it will, when it is at that time ſeparated from the vegetable, yield by itſelf in the ſun, but more ſo by the fire, a good quantity of dephlogiſticated air. The reaſon why the green matter ceaſes at laſt to yield more air is therefore by no means becauſe the water is exhauſted of air; but, on the contrary, becauſe it is too much ſaturated with it, and that with an air hurtful to vegetable life, and becauſe this water has at laſt loſt its own natural air, and together with this air the nouriſhing and phlogiſtic particles which are neceſſary to keep up the full vigour of plants.

If it was the water, and not the vegetable, which furniſhed the dephlogiſticated air, why ſhould the air bubbles not ſettle indifferently on either ſurface of the leaves? In this ſuppoſition, how could that admirable regularity be accounted for, by which all the leaves of a vine and a lime-tree are firſt covered with air bubbles on the under ſide, and all the leaves of *lauro-ceraſus*, at the upper ſurface, whichever ſurface of the leaves is expoſed to the rays of the ſun? Why ſhould the air always ſettle on moſt leaves in the form of bubbles, but never ſo upon the leaves of the *tropæolum majus*, at leaſt during the firſt hours, but always in the form of bags, adhering to the upper edge of the leaves, and detaching themſelves when they are grown to a certain ſize, which never happens with leaves of a vine or lime-tree *; whereas the green ſtalks of this plant (the *tropæolum majus*) are at the ſame time all covered with ſeparate air bubbles? More of theſe remarkable appearances in different plants are to be ſeen in the third ſection of my book on Vegetables.

If the dephlogiſticated air, obtained by means of vegetables in water, was air depoſited from the water, and purified of its phlogiſton, by remaining, during a certain time, in contact with the vegetable, it would follow, that this air would be ſo much the purer the longer it remains in contact with the vegetable; but it is quite the reverſe. The air obtained from the leaves of a vine, to which the air bubbles ſtick a long while before they detach themſelves and riſe up, is never by far ſo much dephlogiſticated as is the air obtained from ſome American plants, out of which the air ruſhes almoſt all in continual ſtreams, as ſo many ſprings, not remaining a ſingle moment upon the leaves. The difference of both airs is ſo great, that I never got dephlogiſticated air from leaves of a vine, lime-tree, and ſuch like, whoſe goodneſs ſurpaſſed 260° (it is commonly a little above 200°); whereas I got commonly, from the above mentioned American plants, an air whoſe goodneſs was of above 300°, ſometimes even of above 350°, in a very fair day, this air being put to the teſt, according to the manner which I have deſcribed before †.

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* I have explained the reaſon of this ſingularity in that plant, in my book, where it is mentioned by the name of *naſturtium indicum*.

† The *ſempervivum tectorum*, which grows almoſt every where upon the roofs of houſes, gives alſo a very great quantity of dephlogiſticated air of an eminent quality. It ſhould ſeem, that all fleſhy plants particularly excel in the quality of yielding fine dephlogiſticated air, and a great quantity of it. The *agave Americana*

If it was the water, and not the plant, which yielded the dephlogiſticated air, the quantity of air obtained would bear, in general, a proportion to the quantity of the water employed; but this is by no means the caſe. The quantity of air bears a proportion to the bulk of the vegetable much more than to the quantity of the water. This is very eaſily to be obſerved with ſome of the above mentioned American plants. It ought to be always obſerved, that if too many leaves are crowded together, they ſhade one another too much; and therefore, in this caſe, the quantity of air obtained will be proportionably leſs, and its quality worſe.

Again: if the dephlogiſticated air, obtained from plants in water, was air diſengaged from the water, it would follow, that a plant ſhut up in a tranſparent glaſs veſſel without any water would yield no air at all, nor increaſe the quantity of air ſhut up with the vegetable. The following experiment, I think, will be ſufficient to convince any one that this is far from being the caſe. I placed in a glaſs tube, hermetically ſealed at one end, a piece of an American plant, called *cereus*; the extremity of this piece, where it was cut from the plant, was tightly ſqueezed in a ſmall glaſs veſſel, in which only as much water was kept as ſeemed to be required to keep the *cereus* in full vigour. I ſmeared the vegetable, and the orifice of the glaſs veſſel all around with ſoft wax, ſo that all communication between the air within the tube and the water within the ſmall veſſel was cut off. I placed this tube inverted in a veſſel filled with quickſilver, keeping a column of ſome inches of

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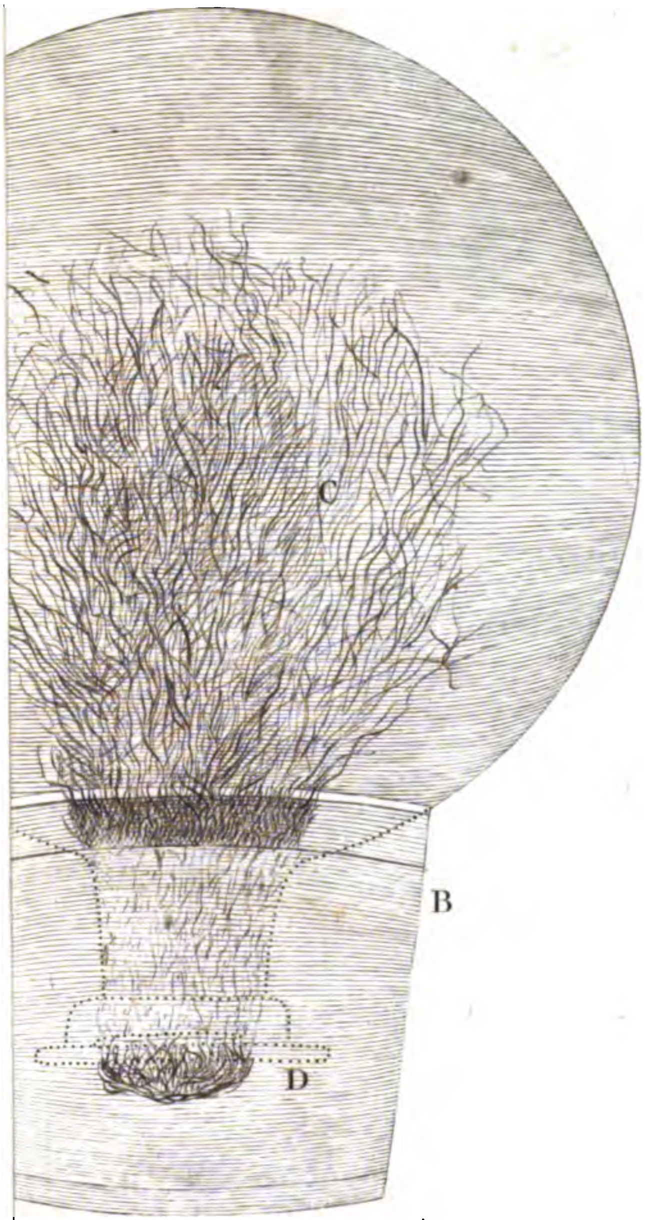
ricana gives ſuch a prodigious quantity of dephlogiſticated air, that, in a fair day, I often got from one ſingle leaf above 150 cubic inches of this air, of the fineſt quality. The *cactus triangularis*, *cereus*, *ſempervivum arborum*, and many others give no leſs air.

the metallic fluid in the tube, to allow the air within the tube to expand by the heat of the ſun without eſcaping. After this apparatus had ſtood during a few hours, in a bright ſun-ſhine, I cooled the whole to the ſame degree it poſſeſſed when it was expoſed to the ſun. This was done by plunging the whole in a tub filled with water, whoſe degree of heat was reſtored to the ſame degree it had before. I found the quantity of air within the tube remarkably increaſed, and ſo far dephlogiſticated that a flame burned in it with an increaſed brightneſs, and that one meaſure of it joined to one meaſure of nitrous air occupied 0.64; whereas the ſame air, before it was ſhut up with the *cereus* was of ſuch a degree of goodneſs, that one meaſure of it with one of nitrous occupied 1.06. Though this experiment may fully ſhew that plants throw out air in the ſun-ſhine, yet if another plant, which does not by nature yield ſo much air as the *cereus* commonly does, is ſhut up in a ſimilar tube, and expoſed to the ſun, the ſame effect will not always be obtained. The reaſon of it is, that plants abſorb a good quantity of common air as their nouriſhment at the ſame time that they throw out dephlogiſticated air. This fact, therefore, if conſidered by itſelf, will not be looked upon as equally demonſtrative with the above mentioned ones. The reſult of this experiment may depend on the more or leſs vigour of the vegetable employed, on the more or leſs brightneſs of the ſun's light, on the more or leſs heat the vegetable receives, &c. A *cereus* being a plant of hot climates may bear more heat than an European plant. All theſe circumſtances, as well as many others, may make the iſſue of this experiment ſometimes ambiguous; but the fact, as I have related it, joined to the above mentioned analogous experiments of Dr. HALES and Mr. BOYLE, will add ſtrength to my aſſertion, *viz.* that vegetables really throw out air in the ſun-ſhine.

If all what I have ſaid hitherto ſhould not be thought ſufficient to take away the prejudice which Dr. PRIESTLEY's fifth volume, and Mr. CAVALLO's book on Air, may have produced in the mind of ſome philoſophers, I ſhould adviſe them to be preſent, at leaſt once, at the moſt beautiful ſcene which they will behold, when a leaf of an *agave Americana*, cut in two or three pieces, is immerſed in a glaſs bell or jar full of pump-water, inverted and expoſed to the ſun in a very fair day in the middle of the ſummer, when this plant is in its full vigour; and when they ſhall have ſeen thoſe beautiful and continual ſtreams of air, which ruſh from ſeveral parts of this vegetable, principally from the white internal ſubſtance of it, I will be anſwerable for their laying aſide all farther doubt about the truth of my doctrine.

After having now demonſtrated, as I think, in the cleareſt manner, that vegetables diffuſe through our atmoſphere, in the ſun-ſhine, a continual ſhower of this beneficial, this truly vital air; and that plants immerſed in water, far from robbing it of all air, impregnate it fully with a better and more ſalubrious air; let us not paſs ſo wonderful, and hitherto not even ſo much as ſuſpected, an operation of nature, without admiring the deſigns of that infinite wiſdom, who has employed ſuch hidden, ſuch wonderful, and at the ſame time ſuch beneficial means to preſerve from deſtruction the living beings which inhabit our earth; and let us conſider, whether it would not be worth while to attempt drawing ſome benefit from this new diſcovery, by making uſe of veſſels of water, in which ſome leaves of vegetables have been expoſed in the ſun-ſhine; by placing ſuch veſſels in our rooms; by ſtirring the water; by



ntaining about 160 cubic inches of water.
Mercury in which the orifice of the spherical veſsel is plunged.
a rivularis.
the Conferva rivularis is attached to keep it in its place.

ſprinkling with it our floors inſtead of uſing for this purpoſe common water; by placing within our houſes, inſtead of flower-pots, diſhes containing ſome *conferva rivularis*, a plant to be met with almoſt every where, ſhooting forth with the utmoſt luxuriancy in all water baſons, in all tubs and veſſels in which water is kept. Is it poſſible, after all this, not to believe, that the Creator has multiplied this vegetable with a ſimilar view to our benefit? This benefit we may now, with ſome confidence, apply to our preſervation, by honouring this vegetable with a place in thoſe of our own rooms which are expoſed to the ſun, and keeping it alive as long as we pleaſe; which may be done by only pouring every day freſh water upon it, and ſqueezing gently now and then out of it the dephlogiſticated air with which the whole maſs ſwells up almoſt as ſoon as the ſun caſts its rays upon it. The water itſelf, in which it has been immerſed, will now, perhaps, be looked upon as too precious to be thrown away as uſeleſs and deprived of that very principal of animal life, of which I have demonſtrated it to be highly pregnant.

XXVIII. *A Microſcopic Deſcription of the Eyes of the* Monoculus Polyphemus LINNÆI. *By Mr.* William André, *Surgeon; communicated by Sir* Joſeph Banks, *Bart. P. R. S.*

Read May 30, 1782.

THE wonderful ſtructure of the eyes of inſects in general, moſt commonly illuſtrated by that of the *Libellula*, or *Dragon-fly*, cannot fail of ſtriking with aſtoniſhment the naturaliſt who inveſtigates the works of the great Creator in his moſt minute productions. According to LEWENHOEK, HOOK, and others, the *corneæ* of moſt inſects are made up of an infinite number of ſmall, tranſparent, horny lenſes, each reſembling, in ſome degree, a ſmall magnifying glaſs. This ſtructure prevails in the *corneæ* of inſects in general; but the *Monoculus Polyphemus*, or *King Crab*, is, among others, an exception to this rule.

The *Monoculus Polyphemus*, or *King Crab*, is a cruſtaceous animal found in all the ſeas ſurrounding the continent of America and the Weſt India iſlands, and which frequently grows to a very large ſize [*]. I ſhall deſcribe ſo much of the *Monoculus* only as is neceſſary to point out the ſituation of the eyes, which have been looked upon as two in number only [†], though in reality they are four. The largeſt piece of the cruſtaceous covering of this animal, when ſeparated from the reſt of the ſhell, has very much the ſhape of a barber's baſon, or the fore-part of a woman's bonnet. The eyes are a part of the ſhell, or, as LINNÆUS expreſſes it, they are *teſtæ innati*[*]. They may be diſtinguiſhed by the terms large and ſmall, or lateral and anterior. If the ſhell were divided fairly in half, the large eyes would be nearly in the center of each piece, and the ſmall ones on the divided edge near the fore-part of the ſhell. The large eyes are at a great diſtance from each other; but the ſmall ones are cloſe together. It will appear hereafter, that the large eyes are made up of a great number of ſmall, tranſparent, amber-like cones, and that the ſmall ones are compoſed of one ſuch cone only; ſo that they may be divided into eyes with many cones, and eyes with a ſingle cone[†]. The large eyes, or thoſe with many cones, appear as two tranſparent ſpots about the ſize and nearly of the ſhape of a kidney bean, the concave edges looking towards each other, and the convex towards the edge of the ſhell. If they be examined attentively, we may diſcern on their ſurface a number of ſmall depreſſions, which point out the center of each cone. The ſmall eyes, or thoſe with a ſingle cone, look like two ſmall tranſparent ſpots, not larger than a pin's head; theſe, from their minuteneſs, are eaſily overlooked, ſee fig. 1. where A.A. ſhew the large eyes, and B.B. the ſmall ones.

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[*] Bossu's Travels, vol. I. p. 368.

[†] LINNÆI Syſtema Naturæ, tom. I. p. 1057.

The appearances which I have deſcribed may be ſeen on the external ſurface of the ſhell with the naked eye; but in order to proceed to a further inveſtigation of the ſubject, the *corneæ* muſt be removed from the ſhell, and applied to a ſingle microſcope with a very ſtrong light.

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[*] This being the caſe, the eyes can enjoy no motion; in which particular, as well as in ſome others, the *Monoculus Polyphemus* differs from the genus of crabs, whoſe eyes are placed on petioles, or ſtalks, and are moveable.

[†] The Greek words πολυς κωνος, and μονος κωνος, would expreſs the ſenſe in a more conciſe manner. *Oculi polyconici et oculi monoconici*.

The internal ſurface of the large eyes, examined with the microſcope, is found to be thick ſet with a great number of ſmall, tranſparent cones, of an amber colour [*], the baſes of which ſtand downward, and their points upwards next the eye of the obſerver [†]. The cones in general have an oblique direction, except ſome in the middle of the *cornea*, about thirty in number, the direction of which is perpendicular. The center of every cone being the moſt tranſparent part, and that through which the light paſſes; on that account the perpendicular or central cones always appear beautifully illuminated at their points. In a word, they are all ſo diſpoſed as that a certain number of them receive the light from whatever point it may iſſue, and tranſmit it to the immediate organ of ſight, which we may reaſonably ſuppoſe is placed underneath them; but this laſt circumſtance can only be determined in a recent ſubject, which I have never been ſo lucky as to ſee. The cones are not all of the ſame length; thoſe on the edges of the *cornea* are the longeſt, from whence they gradually diminiſh as they approach the center, where they are not above half the length of thoſe on the edges, ſee fig. 2.

As theſe cones ſo eaſily tranſmit the light through their ſubſtance, when I firſt examined them I thought they were tubes; but I have ſince viewed them broken in different directions, and am convinced they are ſolid tranſparent bodies. If they be viewed with a deep magnifier, every cone appears divided tranſverſely by two or three internal *ſepta* or partitions.

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[*] I have made ſome attempts to aſcertain their number, and think they amount to about 1000.

[†] This muſt be reverſed if the eye be conſidered in its natural poſition.

This appearance is owing to the cones themſelves being made up of ſeveral cones, one within another, the *ſepta* or partitions being nothing more than the *apices* or points of the external cones; but this will be further explained by conſidering that the *cornea* of the *Monoculus* may be divided into layers, the number of which, however, I cannot aſcertain; but I once met with a *cornea* in which the internal layer and its cones was ſeparated from the external *lamina* and their cones. A portion of the internal layer is ſhewn fig. 4.; and the cones, very much magnified, with their *ſepta* or partitions, are exhibited fig. 5.

It is very well known, that all cruſtaceous animals depoſit their ſhells once a year, and are left with a ſoft, tender covering, which, after ſome time, acquires the hardneſs of the former ſhell. As the *cornea* in theſe animals is a part of the ſhell, it is reaſonable to ſuppoſe, that the internal layer is left with the ſoft covering, containing the rudiments of the future *cornea*; and this is the more probable, from what I have before obſerved, that I have met with an eye where the internal layer was ſeparated from the more external ones, ſee fig. 4.

The ſtructure of the ſmall eyes being leſs elaborate than that of the large ones, their internal appearance, when placed in the microſcope, will be deſcribed in a few words. They conſiſt of an oval, tranſparent, horny plate, of an amber colour, in the center of which ſtands a ſingle cone, through which and the oval plate the light paſſes, ſee fig. 3 *.

Having thus deſcribed, as conciſely as poſſible, the ſingular mechaniſm of the *corneæ* of the *Monoculus*, I ſhall add a few words concerning their uſe. The lenticular ſtructure of the *corneæ* of inſects in general certainly aſſiſts in condenſing or ſtrengthening the light in its paſſage to the immediate organ of ſight. It is probable, that the cones in the *Monoculus* have the ſame effect. Whether they anſwer that purpoſe, in a more or leſs perfect manner than the lenſes in the generality of inſects, is what I cannot take upon me to determine.

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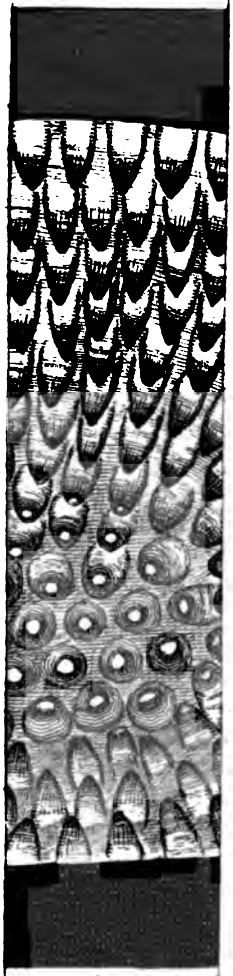
* The ſmall eyes are analogous to thoſe ſmall eyes of other inſects which entomologiſts have called *ſtemmata*.

EXPLANATION OF THE PLATE.

Fig. 1. The *Monoculus Polyphemus*.
AA. The large eyes.
BB. The ſmall ones.
Fig. 2. One of the large eyes magnified.
Fig. 3. One of the ſmall eyes magnified.
Fig. 4. A portion of the internal layer magnified.
Fig. 5. The cones magnified with their *ſepta* or partitions.

Fig. 2.



olyphemus

Fig. 5.

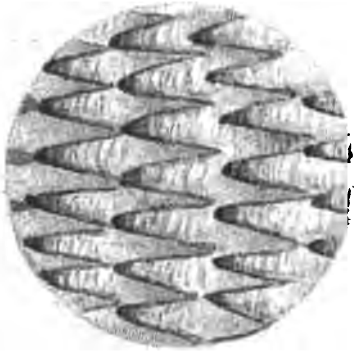


Fig. 3.

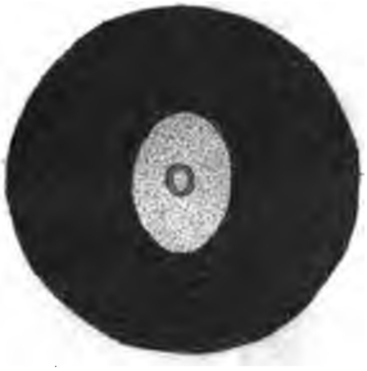
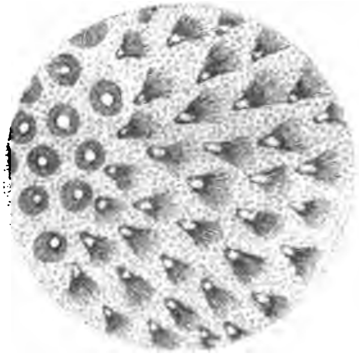


Fig. 4.



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ERRATA.

Page.	Line.	
72.	2.	*for* comunicated *read* communicated
77.	12.	*for* mercury removed without cauſing this to riſe *read* removed without cauſing the mercury to riſe
81.	9.	*for* ſmaller towards the middle *read* larger towards the middle
391.	—	the laſt line, *for* $-$ *read* $+$
403.	12.	*for* -54 *read* -46
408.	3.	from the bottom, *for* $\frac{rn+m}{n}$ *read* $\frac{n}{rn+m}$.

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